Fighting Silent Killers: How India's Public Healthcare Staffing Expansion Saves Lives by Improving Access and Market Quality

Patrick Agte (Yale) Jitendra Kumar Soni (IAS)*

December 10, 2024

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Abstract

Millions of adults in low- and middle-income countries die from treatable conditions every year. This paper highlights that an understaffed public healthcare system contributes to high premature mortality, both directly by affecting public provision and indirectly by allowing low-quality private providers to remain competitive. We evaluate a large-scale reform to India's public healthcare system that adds a mid-level healthcare worker to village clinics. Exploiting quasi-experimental variation due to assignment rules, we find that adding a worker reduces all-age mortality in the catchment area by 10% within two years, making the reform highly cost-effective. Eighty percent of the decline is attributable to a decrease in deaths of adults aged 56+, increasing their life expectancy by at least three months. We conduct audit visits, patient exit interviews, and provider surveys to study mechanisms and find that the program improves performance and service availability in the public sector and also induces private providers to increase their quality. To quantify the importance of each of these channels and evaluate counterfactual policies, we estimate a structural model of patient demand. Ten percent of the decrease in all-age mortality can be attributed to the private sector response, while the remaining 90% is due to simultaneous improvements in public sector quality and access. Only improving public sector quality or access in isolation has limited effects. Model estimates further demonstrate large heterogeneity in predicted treatment gains; we show that an optimal reallocation of the new providers that accounts for local market conditions could achieve a substantially greater reduction in mortality.

*Contact: patrick.agte@yale.edu. We are especially grateful to Janet Currie, Thomas Fujiwara, and Rohini Pande for their encouragement, advice, and patience. We are particularly thankful to Christopher Neilson for detailed comments. We express gratitude for the support from the Government of Rajasthan. We thank Ashi Sharma, Sandhya Seetharaman, and Gulshan Banas for their excellent assistance. We are also thankful for valuable help from Khushi Baby and the WISH Foundation. We thank Arielle Bernhardt, Jishnu Das, Mateus Ferraz Dias, Anne Karing, Seema Jayachandran, Natalia Rigol, Sagar Saxena, Christiane Szerman, and seminar participants at Princeton, Yale, IEG, NHSRC, and NEUDC for helpful advice and comments. This project was made possible with funding from Princeton, Yale, NSF, the Weiss Fund, and J-PAL. Soni was involved in the implementation of this reform; however, all views and errors are solely ours, and this paper does not necessarily represent the view of the Government of Rajasthan.

1 Introduction

Nearly 40% of deceased adults in rural India do not receive medical attention before their death (NSS 2017–2018). When they seek healthcare, they typically choose between traveling to the nearest town or staying in the village and either visiting an understaffed public clinic or an informal private provider with limited medical qualifications.[1] Either way, healthcare quality is low (Das et al., 2016). Demand-side barriers related to lack of information, financial constraints, and behavioral biases of patients contribute to this situation, resulting in few incentives for private providers to improve their quality.[2] Without the ability to enforce quality standards, governments in low state capacity settings can often only intervene by directly investing in the public sector. However, investments might not be sufficient to overcome high rates of shirking in the public sector. Furthermore, even if public services improve, the effects on health outcomes depend on how much patients value these changes, where they would seek healthcare otherwise, and how private providers respond to heightened competition.[3]

This paper studies how a large-scale staffing reform to India's public primary healthcare sector affected mortality outcomes and the behavior of patients and providers in the healthcare market. The reform was implemented in a staggered fashion and involved assigning one additional healthcare worker to every Indian village clinic. Before the reform, these clinics were only staffed by a midwife, who provided maternal and child health as well as outpatient care services. The new healthcare workers are more highly qualified non-physician practitioners, mandated to provide basic acute and preventive healthcare services for adults. By 2024, 138,257 new healthcare workers were added to village clinics across India, impacting healthcare delivery for over 750 million people (MHFW, 2024).[4]

[1]Das et al. (2022) find that 75% of Indian villages have an informal private provider but only 6% have a private provider with a formal MBBS degree.

[2]Information frictions include the limited observability of provider quality (Wagner et al., 2023) and an underestimation of the returns to receiving high-quality healthcare (Dupas, 2014). Behavioral biases cover overoptimism (Kim and Niederdeppe, 2013), present bias (Bai et al., 2021), and information avoidance (Oster et al., 2013; Li et al., 2021).

[3]While better public healthcare might crowd in private provider quality, an increase in healthcare market competition could also worsen patient outcomes by increasing overmedication (Bennett et al., 2015; Currie et al., 2023), market segmentation (Atal et al., 2024), or private provider exit (Dinerstein and Smith, 2021).

[4]The average population covered by a village clinic is 5,624 people (MHFW, 2022). In Rajasthan, catchment areas are smaller and cover, on average, 3,000 people.

Our analysis has two parts. First, we exploit quasi-experimental variation in the allocation of new healthcare workers in Rajasthan, one of India's largest and poorest states, to assess the direct impact of the reform. Second, we estimate a structural model of patient demand to study mechanisms and evaluate potential gains from reallocating the new healthcare workers based on local market conditions. We rely on three data sources. Our large-scale administrative health data cover the universe of villages in Rajasthan for up to two years after the reform. We supplement the administrative data with two rounds of original survey data on public providers, private providers, and households across 193 villages. During the second round of data collection, we further conduct audit visits and patient exit surveys at sample village clinics to obtain information on healthcare access and provider performance. Finally, in collaboration with a local NGO, we collect data on households' provider choices as part of a healthcare household census.

In the first part of the paper, we exploit the staggered rollout of the healthcare reform in a matched difference-in-differences design that is informed by the rules of the healthcare worker assignment process. Due to budget constraints, the government of Rajasthan could fill just two-thirds of eligible worker vacancies in the first wave of program implementation (March 2022). The decision of which village clinics within a district received a new healthcare worker was ad-hoc: local government officials had a single day to decide on assignments and the only information provided to them was the healthcare worker's place of residence and clinic locations. This resulted in quasi-random variation in assignments to village clinics conditional on a clinic's location. Consistent with our knowledge of the assignment rule, we find that matching treatment and control group clinics based on a propensity score function that uses only district fixed effects and the clinic's distance to the district and subdistrict headquarters is sufficient to create balance across other observable clinic characteristics.

We have three main findings from our quasi-experimental analysis. First, using statewide data on mortality outcomes, we show that treated areas that received a new healthcare worker experienced a 10% reduction in all-age mortality rates within the first 24 months. Eighty percent of this decline can be attributed to a decline in elderly (age 56+) deaths, implying an increase in their life expectancy by 3 to 16 months.[5] The mortality outcomes for

[5]Three months is the minimum increase necessary to achieve the observed reduction in elderly mortality

other age groups are not significantly impacted.[6] Results from our household survey further indicate a decline in hospitalizations, also concentrated among the elderly. These findings are robust to various sensitivity tests, including an alternative difference-in-differences design that compares treatment group clinics with the closest control group clinics in the area.[7] We also observe declines in mortality outcomes using a separately maintained dataset on deaths from the civil registration system.

Second, we document that part of the effects comes from better healthcare provision in the public sector. Results from hypothetical medical vignettes and patient exit surveys show that the new healthcare workers improved checklist completion rates and patient-provider interactions. The labor inputs also increased access to healthcare services, with treated public clinics being 64% more likely to be open during unannounced audit visits than control group clinics. An increase in monthly patient loads by 58% at facilities with a new healthcare worker indicates that patients value these changes. Using state-wide data from a healthcare census of households, we find that the increase in patient visits is driven entirely by patients who would not otherwise seek healthcare services. A higher number of patients diagnosed with acute heart diseases (e.g. heart attacks) and epilepsy as well as with hypertension and diabetes at treated facilities suggest that the additional healthcare workers improved both acute and preventive care. Reassuringly, we find that the reform did not divert attention from existing maternal and child health services.

Our third finding is that private providers in treated areas respond to the public sector reform by improving their quality, evidenced by increased enrollment in medical degrees and better vignette performance. Consistent with a decrease in market power driving the results, these effects are especially pronounced among providers that were the only private providers in the area at baseline. We observe no change in the number of private providers or their patient load, prices, or use of antibiotics and injections.

In the second part of the paper, we estimate a structural model of patient demand to

within two years. The increase of 16 months is based on a Gompertz mortality model and assumes that the reform reduces the base mortality rate by 10%.

[6] Our administrative data provides aggregate information on deaths for five age groups: infants (< 1 year), children ($1 - 4$ years), adolescents ($5 - 14$ years), adults ($15 - 55$ years), and the elderly (56+ years).

[7] Similar levels and trends in the pre-period provide further evidence against concerns that differential trends between treatment and control units could be driving our results.

evaluate optimal staffing policies and analyze how much different mechanisms contribute to the decline in mortality. For the model, we combine aggregate market shares with individual-level choice data to allow for observed and unobserved heterogeneity in patient preferences over spatially differentiated providers. To separate the increase in healthcare quality from an increase in healthcare access, we exploit variation in the availability of person-hours at public facilities related to whether healthcare workers live in the village where the facility is located. We argue that this variation is related to personal circumstances and support this claim by showing that catchment area characteristics do not predict worker residence locations. Differences in the locations of medicine suppliers across private providers further generate plausibly exogenous variation in prices, and the covariance in provider prices between a patient's first- and second-choice helps us identify unobserved preference heterogeneity.

Model estimates indicate that, given the observed effects on mortality outcomes, patients undervalue provider quality when choosing whether and where to seek healthcare. Counterfactual simulations show that only increasing the quality of public village clinics, but keeping public sector person-hours and private provider quality constant, would thus only achieve 33% of the observed decline in mortality. Only increasing person-hours at public clinics would also have little effect on mortality, partly because infra-marginal patients do not benefit in this case. By contrast, simultaneously increasing public sector quality and person-hours achieves 90% of the observed decline in mortality, while changes to private provider quality explain the remaining 10%.[8]

We further demonstrate that large heterogeneity in the marginal effectiveness of allocating healthcare workers to specific clinics and that the government could have substantially increased the reform's impact on mortality outcomes by taking local market conditions into account when making worker assignments. The observed government assignment yields results comparable to random allocation, whereas the optimal assignment would result in a 33% greater decline in mortality by prioritizing clinics that are more distant from towns, have lower baseline quality, serve larger catchment populations, and are situated near private providers with greater market power. We also show that reallocation schemes that

[8] We also evaluate another commonly discussed healthcare reform, the closure of private providers, and find that such a policy would decrease average health outcomes, even after the public sector staffing expansion.

use specific rules based on observable information or that account for political feasibility constraints related to how far new workers are willing to relocate still lead to considerable gains. Changes to optimal assignments based on the weight given to the outcome of poor households highlight the trade-off between targeting average impacts or health equity.

We estimate that the large-scale primary healthcare reform is highly cost-effective even in its current form. Using USD 100,000 as the value of a statistical life year, we find that the reform generated at least USD 6.84 in private benefits for every government dollar spent. If we account for the decline in hospitalizations, the reform could even pay for itself by reducing future government spending.

This paper contributes to multiple bodies of work. First, we add to the literature on the determinants of elderly health outcomes in low- and middle-income settings. Previous research has focused on demand-side constraints to healthcare utilization (Dupas, 2011). There is scant evidence, however, on supply-side interventions and those that do study this topic typically examine their impacts on infant health (Carrillo and Feres, 2019; Okeke, 2023; Björkman and Svensson, 2009). As mortality profiles are changing with aging populations, it becomes increasingly important to study adult mortality. Recent studies demonstrate that new healthcare facilities (Mora-García et al., 2024) and cash transfers (Barham and Rowberry, 2013) can reduce adult mortality, whereas evidence on the impact of health insurance is mixed (Chen et al., 2007; Sood et al., 2014; Gruber et al., 2023; Malani et al., 2024). Our findings show how a large-scale reform that strengthened existing public facilities was effective in reducing elderly mortality.

Second, we speak to the literature on the personnel economics of the state (Finan et al., 2017). Previous work studies how to improve the performance of existing public sector workers and often finds disappointing results due to high rates of absenteeism and shirking (Banerjee et al., 2008; Dhaliwal and Hanna, 2017).[9] By contrast, increasing the number of personnel can directly augment state capacity as long as workers exert a minimum level of effort. Consistent with this, recent studies demonstrate that labor inputs effectively enhance government service provision (Duflo et al., 2015; Björkman et al., 2019; Ganimian et al.,

[9]One notable exception includes several studies that demonstrate how community monitoring can enhance the performance of existing workers by improving accountability (Björkman and Svensson, 2009; Christensen et al., 2021; Mohanan et al., 2020).

2024). In healthcare, the evidence is mixed since health outcomes are not only affected by the number of workers but also by the substitutability of different worker types (Carrillo and Feres, 2019; Okeke, 2023). We contribute to this literature by showing how governments faced with physician shortages and tight budgets can use non-physician practitioners – a type of mid-level healthcare worker that is already used in 37 countries across Africa and Asia (Desai et al., 2020) – to increase access to basic healthcare services and improve patient-provider interactions.[10]

Finally, our results speak to the literature that studies patient demand and interactions between public and private sectors in healthcare markets in low- and middle-income countries. Prior work on market interactions focuses primarily on education (Dinerstein et al., 2023; Andrabi et al., 2024; Allende, 2021; Neilson, 2021).[11] In healthcare, limited data availability makes it difficult to study patient preferences and private sector behavior.[12] Using novel survey data on provider attributes and the revealed choices of patients, we show large gains from simultaneously improving public sector quality and access and that an increase in competition induces private providers to invest in quality upgrades without increasing overmedication or provider exit. By demonstrating how a reallocation of healthcare workers could have improved health outcomes, we also speak to the broader literature on misallocation of inputs in healthcare markets (Hsiao, 2022; Chandra et al., 2023; Lim, 2023).

The rest of this paper proceeds as follows. Section 2 describes the institutional context and conceptual framework. Section 3 discusses our empirical strategy. Section 4 presents our main results. Section 5 examines mechanisms and counterfactual policies using a structural model of patient demand. Section 6 discusses the findings and the cost-effectiveness of the reform. Section 7 concludes.

[10] While the average global physician density was 17.2 physicians per 10,000 people in 2020, India had 7.3 physicians per 10,000 people (WHO, 2024). India's public health expenditure was also only 1.35% of its GDP. By contrast, public health expenditure is more than 10% of GDP in the United States and Germany.

[11] An important distinction from education markets is that private providers in rural healthcare markets often do not differentiate themselves through quality but through better access to care, which may influence how they respond to improvements to the public option. Moreover, concerns related to moral hazard and patient demand for unnecessary and potentially harmful drugs that are unique to healthcare could change how an increase in competition affects the private sector (Bennett et al., 2015; Currie et al., 2014, 2023).

[12] Previous studies provide descriptive evidence on the quality of public and private providers in India and other countries (Das et al., 2008, 2016; Banerjee et al., 2023) and find that training and regulation interventions can improve private sector quality (Das et al., 2016; Bedoya et al., 2023).

2 Background

We begin by providing details about the reform to the healthcare sector. We then present a conceptual framework to analyze the expected treatment effects on health outcomes.

2.1 Healthcare in Rural Rajasthan

Our analysis focuses on Rajasthan, the seventh most populous state in India. Rajasthan is one of the poorest states in the country, ranking 27th out of 33 states in 2019. Around one-third of the rural population is poor and many households have limited access to healthcare providers.[13] According to data from a recent healthcare census, 36% of household members who were sick in the past 30 days did not receive any healthcare.[14]

Those who do visit a healthcare provider usually have the option of visiting a subcenter (public village clinic) or a private healthcare provider in their village or traveling to the nearest town to visit a public *primary health center* (PHC). Subcenters are the lowest level of primary care in the country and are staffed by an *Auxiliary Nurse Midwife* (ANMs) with a two-year diploma.[15] ANMs primarily focus on maternal and child healthcare services but have evolved into multipurpose healthcare workers over time. At the point of the study, they are also supposed to provide a wide range of other healthcare services, including basic outpatient care and screening for chronic diseases. The expanded range of expected services leaves many of them overburdened.[16] Since the ANMs perform most of their activities in the field, the physical subcenter building is rarely staffed.[17] Patients can alternatively travel to the nearest PHC, the second level in the public primary healthcare system. PHCs are staffed by a physician and are located, on average, 7km away from a subcenter. Healthcare services, tests, and medicines at subcenters and PHCs are free for all patients.[18]

[13] The poverty rate is based on the 2011 imputed poverty share in the SHRUG data (Asher et al., 2021).

[14] Healthcare utilization rates vary by the type of symptom, but even among individuals with severe symptoms (vomiting, fatigue, or difficulty breathing), 19% did not visit a healthcare provider.

[15] ANMs also receive support from three community health workers.

[16] In our baseline survey, 69% of ANMs say that too much work is allocated to them and 57% say that they do not have sufficient time to complete their work.

[17] Only 42% of facilities in the control group were open during unannounced visits in our sample area.

[18] Rajasthan was one of the first states that implemented a free medicine scheme in India. Rajasthan also spends more on healthcare than other states in general. In 2020–21, the state allocated 7.1% of the government budget to healthcare, whereas other states spent, on average, 5.3%.

Since access to the public healthcare system is often unreliable, many patients prefer to visit (informal) private healthcare providers instead.[19] Among the subcenters in our survey sample, 58% have at least one private provider in the catchment area.[20] Most of these providers have limited medical qualifications, with 69% of our sample providers having less than a bachelor's degree. These providers prescribe and dispense medicines with a markup, leading to potential overmedication due to moral hazard (Currie et al., 2014; Das et al., 2016). The median fee for medicine and consultation at a private provider in our sample is INR 100 (1.20 USD), equal to 5% of monthly household income per capita. Previous research has shown that patients prefer private providers because they are more available and tend to be more aggressive in dispensing drugs (Gautham et al., 2011; George and Iyer, 2013).[21] Many of these providers face little competition: 38% of private providers in our survey sample are the only private providers in the subcenter catchment area.

To strengthen the provision of public primary healthcare, the Government of India announced the Health and Wellness Center reform in September 2018 as part of the Ayushman Bharat initiative.[22] The reform aims to address the changing burden of disease by providing comprehensive primary healthcare in rural areas and converting 150,000 subcenters and PHCs into Health and Wellness Centers. The operational guidelines for the reform mentioned that the changes were motivated by global evidence that comprehensive primary healthcare "reduces morbidity and mortality at much lower costs and significantly reduces the need for secondary and tertiary care" (MHFW, 2018).

The key component of the Health and Wellness Center Reform is the creation of a new cadre of mid-level health providers, known as *Community Health Officers* (CHOs).[23] CHOs

[19] One-fifth of our household survey sample report that they primarily seek healthcare from a private provider in their village.

[20] Private provider presence is positively correlated with distance to PHC and catchment population but negatively correlated with the catchment area poverty share.

[21] While median private provider in our sample is open for 56 hours per week, the median ANM only works 36 hours per week (and most of this time is spent outside of the facility). Private providers are also more likely to give injections and antibiotics in medical vignettes. Private provider quality, either measured by medical vignettes or length of medical degree, is very similar to ANM quality in our sample.

[22] The other component of the initiative was the expansion of public health insurance through the Pradhan Mantri Jan Arogya Yojana (PMJAY) scheme. In Rajasthan, the government agreed to implement the PMJAY scheme in June 2019. A separate health insurance scheme under the name of Chiranjeevi Yojana was launched in May 2021.

[23] Appendix D describes additional details of the reform.

are required to have a three- or four-year degree in nursing and work alongside the existing ANMs. The main mandate of the CHOs is the provision of basic adult outpatient care and screening for chronic diseases at the subcenter level. Their payments consist of a fixed component as well as performance-based incentives for 15 indicators.[24]

2.2 Conceptual Framework

To understand how adding CHOs to subcenters could affect health outcomes, we present a model of patient demand.[25] We start with a stylized version in this section to emphasize the main forces at play. In Section 4, we extend the model and take it to the data.

Let J^m be the number of healthcare providers available in market m. Whenever patient i gets sick, the patient needs to choose which of these healthcare providers to visit or whether not to seek healthcare at all. We characterize patients by their poverty status and their location. Locations consist of the village or town in which the PHC is located as well as the subcenter villages that are connected to the corresponding PHC in market m. The main provider characteristics are their location, person-hours (h_j), quality (q_j) and price (p_j). Additional provider characteristics are captured by x_j. We assume that patient preferences over distance, quality, and price differ by poverty status. Patients also have random preference shocks for providers (ϵ_{ij}) that follow an i.i.d. Type 1 extreme-value distribution.

A patient i's utility from seeking healthcare at provider j is

$$u_{ij} = \beta_i^q q_j + \beta^h h_j - \alpha_i p_j - \lambda_i d_{ij} + \beta x_{jt} + \epsilon_{ij}, \tag{1}$$

with $\beta_i^q = \bar{\beta}^q + \beta_1^q poor_i$, $\alpha_i = \bar{\alpha} + \alpha_1 poor_i + \nu_i$, and $\lambda_i = \bar{\lambda} + \lambda_1 poor_i$, where $poor_i$ is an indicator variable for whether the patient comes from a poor household and d_{ij} is the distance between patient i's and provider j's locations. $u_{i0} = \epsilon_{i0}$ represents the utility from not seeking any healthcare.

Patients choose the provider j that maximizes their utility. We can write the share of non-poor patients who live in location l and select provider j as a function of provider quality,

[24] The performance-based incentives cover a list of 15 service-based indicators at the subcenter level. ANMs and community health workers also receive smaller incentive payments. See Appendix Table A1 for details.

[25] The model follows a framework that was developed by Neilson (2021) to estimate demand for schools in Chile.

prices, person-hours,and parameters $(\mathbf{q},\mathbf{p},\mathbf{h},\theta)$:

$$s^l_{j,nonpoor}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)=\frac{exp(\bar{\beta}^q q_j+\beta^h h_j-\bar{\alpha}p_j-\bar{\lambda}d_{lj}+\beta x_j)}{\sum_{n\in J^m} exp(\bar{\beta}^q q_n+\beta^h h_n-\bar{\alpha}p_n-\bar{\lambda}d_{ln}+\beta x_n)+1} \tag{2}$$

We can similarly write the share of poor patients who live in location l and select provider j and call it $s^l_{j,poor}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)$. To get total market shares, we sum over the different locations in each market and over each patient type. The population shares of each location for poor and non-poor patients in market m is given by w^l_{poor} and $w^l_{nonpoor}$, such that their total sums are equal to one ($\sum_l^{L_m} w^l_{poor}=1$ and $\sum_l^{L_m} w^l_{nonpoor}=1$), where L_m is the total number of locations in market m. Similarly, the poverty share in market m is given by pov_m. The total market share of provider j is given by

$$s_j(\mathbf{q},\mathbf{p},\mathbf{h},\theta)=pov_m\sum_l^{L_m} w^l_{poor}s^l_{j,poor}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)+(1-pov_m)\sum_l^{L_m} w^l_{nonpoor}s^l_{j,nonpoor}(\mathbf{q},\mathbf{p},\mathbf{h},\theta) \tag{3}$$

We assume that subcenters and PHCs have fixed characteristics that are determined by the government. By contrast, private providers strategically choose prices, quality, and person-hours, and decide whether to exit the market to maximize profits. We further assume that there is a direct relationship between health outcomes and the average healthcare quality $\bar{q}$ chosen by patients.

$$\bar{q}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)=\sum_j s_j(\mathbf{q},\mathbf{p},\mathbf{h},\theta)q_j, \tag{4}$$

with $q_j=0$ if patients choose not to seek healthcare at all.

To understand how adding CHOs to subcenters can affect health outcomes, we start by considering a market that only has two options: a subcenter or no healthcare at all. We assume that adding a CHO is equivalent to improving subcenter quality.[26] The intervention

[26] Adding a CHO to subcenters also improved access to public healthcare services by increasing person-hours. Whether an increase in subcenter quality or person-hours has a larger effect on average healthcare quality depends on patient preferences for these two attributes. We further note that, when we only consider an increase in person-hours, infra-marginal patients do not benefit from the reform since the second term in equations (7)-(9) disappears.

would then have the following effect on average healthcare quality:

$$\frac{d\bar{q}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}} = \frac{ds_{shc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}}q_{shc} + s_{shc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta) \tag{5}$$

The first term, $(ds_{shc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)/dq_{shc})q_{shc}$ captures the change in quality for patients who switch to subcenters once subcenter quality increases, while the second term, $s_{shc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)$ captures the increase in quality for inframarginal patients who would choose the subcenter even in the absence of the increase in subcenter quality. Assuming that patients like quality (and are able to observe it), higher subcenter quality results in an increase in the subcenter market share. Since both terms would then be positive, an increase in subcenter quality would result in better average healthcare quality and improved health outcomes.

We next consider a scenario in which patients also have the option to travel to a nearby town to visit a PHC that provides higher healthcare quality than the subcenter ($q_{phc} > q_{shc}$):

$$\frac{d\bar{q}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}} = \frac{ds_{shc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}}q_{shc} + s_{shc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta) + \frac{ds_{phc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}}q_{phc} \tag{6}$$

The third term, $(ds_{phc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)/dq_{shc})q_{phc}$, captures patients that switch away from PHCs. Improving subcenter quality would increase the market share of the subcenter and decrease the market share of the PHC. Whether the net effect on average healthcare quality is positive or negative depends on the quality difference between the subcenter and the PHC, the distance between the subcenter village and the PHC town, and patient preferences.

Finally, we also consider a scenario in which a private provider is available as well:

$$\begin{aligned}\frac{d\bar{q}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}} = {} & \frac{ds_{shc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}}q_{shc} + s_{shc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta) + \frac{ds_{phc}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}}q_{phc} + \\ & \frac{ds_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}}q_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta) + \frac{dq_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)}{dq_{shc}}s_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta),\end{aligned} \tag{7}$$

where the fourth and fifth terms capture changes in average quality through changes in private sector market shares $[(ds_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)/dq_{shc})q_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)]$ and through changes in private sector quality, $[(dq_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)/\ dq_{shc})s_{priv}(\mathbf{q},\mathbf{p},\mathbf{h},\theta)]$. The inclusion of the private sector further complicates the effect of an increase in subcenter quality on average healthcare quality. Increasing subcenter quality could crowd in private provider quality by creating competitive pressure and reducing market power. However, the change in subcenter quality could also lead to market segmentation, in which private providers focus on providing lower-quality care to patients who are insensitive to quality and value other provider characteristics. The equilibrium depends on the sensitivity of the market shares with respect to provider quality, which in turn depends on the distribution of patient preferences. Private provider exit could also either improve average quality by forcing more patients to choose subcenters and PHCs or worsen average quality if most of the patients who would have preferred to continue visiting the private provider choose not to seek healthcare at all in the absence of the private provider.

Taken together, the conceptual framework has the following implications:

1. *The subcenter market share is weakly increasing with subcenter quality.*

2. *The effect of an increase in subcenter quality on average healthcare quality is ambiguous and depends on private sector responses and patient substitution patterns.*

It is thus important to study the effects of adding CHOs to subcenters empirically. To do this, we first examine average treatment effects using quasi-experimental variation in the rollout of the reform. In the second part, we take the framework to the data and estimate a model of patient demand that allows us to separately shut down each channel and analyze optimal staffing policies.

3 Data and Empirical Strategy

In this section, we start by describing our data sources. We then present the empirical strategy and first-stage results.

3.1 Data

We combine large-scale administrative data on all public primary healthcare facilities in Rajasthan with primary survey data on 193 subcenters in Udaipur district.

3.1.1 Administrative Data

Our primary outcomes come from the Pregnancy, Child Tracking and Health Services Management System (PCTS) portal. We use the portal to obtain aggregate information on healthcare services and deaths at the facility-month level from April 2019 until March 2024. The portal contains information on patient visits and the number of deaths across five age categories: infant deaths (<1 year), child deaths (1–4 years), adolescent deaths (5–14 years), adult deaths (15–55 years), and elderly deaths (56+ years). The reporting covers all deaths of residents in the catchment area, even if the death occurred somewhere else (e.g., at a district hospital). We further use the PCTS portal to get data on five maternal and child healthcare indicators.[27] Importantly, PCTS reporting is always done by the ANM, even after a CHO is added to the subcenter, ruling out that any differences in indicators due the CHOs could be attributed to a change in the reporting person.

We also obtained access to data from the Community Health Integrated Platform (CHIP), a healthcare-focused household census of Rajasthan that Khushi Baby, a local NGO, developed, and that is collected through community health workers. As the census was implemented, we added questions on the healthcare provider choices for all household members who had at least one symptom in the past 30 days.[28] We obtain information on CHO assignments through the Health and Wellness Center Portal and information on 2011 catchment area characteristics through the Socioeconomic High-Resolution Rural-Urban Geographic Platform for India (SHRUG) (Asher et al., 2021). Finally, we received separate information on elderly deaths at the gram panchayat (village council) level from April 2021 until March 2023 through the Rajasthan Civil Registration System (Pehchan). These death records are managed by village councils and maintained independently from the ANM's PCTS records.

[27] We use the five indicators to generate a maternal and child health services index. The five service indicators are the number of pregnant women with at least 4 prenatal care visits, the number of pregnant women who received 360 calcium tablets, the number of pregnant women who received their first tetanus shots, the number of women getting a postpartum check-up seven days after delivery, and the number of fully vaccinated children (aged 9–11 months).

[28] This data is only available for the post-periods since we added the provider choice questions in August 2023.

3.1.2 Survey Data

We supplement our analysis of administrative data with two rounds of primary survey data on ANMs, CHOs, private providers, and households that we collected across 193 subcenters in four subdistricts in Udaipur district. We conducted the endline surveys 9-12 months after the CHOs were added to treated subcenters.[29] In our provider surveys, we obtained information on facility characteristics, their medical knowledge through two vignettes, and, for private providers, prices. We create a quality index based on the provider's medical degree and adult asthma vignette performance.[30] Among subcenters that had at least one private provider in the catchment area at baseline, we conducted a phone survey with 513 households to collect information on health outcomes and healthcare utilization for all household members. Appendix Table A2 shows that we obtained similar baseline and endline completion rates for all surveys across the treatment and control groups.

As part of our endline activities, we further visited each sample subcenter without prior announcement for a full day to measure facility opening rates. During these visits, we also conducted exit surveys with all patients who visited the subcenter on that day to collect information on patient satisfaction and other measures of provider quality. Finally, we implemented endline surveys with physicians at 49 PHCs that are linked to our sample subcenters to benchmark the knowledge of CHOs and obtain information on PHC infrastructure. Additional details on each survey component can be found in Appendix C.

3.2 Empirical Strategy

Our empirical strategy exploits where the first cohort of CHOs in Rajasthan was assigned. We next describe how the assignment decisions were made.

[29] Appendix Figure A1 shows the timing of the different surveys.

[30] We also conducted a child dysentery vignette but do not include it in our quality index since the new CHOs primarily provide outpatient care for adults. Both medical vignettes are based on patient cases developed by Das and Hammer (2005) and Das et al. (2016). We measure vignette performance based on their checklist completion rate and whether they provide the correct treatment to the patient. We follow Das et al. (2016) and classify referrals as correct treatment.

3.2.1 Reform Rollout in Rajasthan

The first cohort of CHOs was assigned to subcenters at the end of March 2022.[31] At this point, 6,419 CHOs had been hired and passed the final exam.[32] The government then had to decide to which of the 10,016 eligible subcenters the new providers would be assigned.[33] The assignment decisions were implemented in two stages. In the first stage, CHOs were asked to rank districts according to their preferences. Assignments were then made based on their exam scores. In the second step, *Chief Medical and Health Officers*, the leading health officials at the district level, were asked to do the assignments within districts. All of the 33 officials in Rajasthan were requested to visit the state headquarters in Jaipur for a day to do the assignments. On that day, the average district official had to allocate 195 CHOs across 304 subcenters, leaving 109 subcenters vacant. The only information that the district officials received was (i) a list with the names and residential addresses of the CHOs and (ii) a list with the subdistrict and village names of the subcenters in the district that had been converted to Health and Wellness Centers. The only instructions given to the officials were to place the CHOs close to their homes and to finish the task by the end of that day.

We conducted qualitative interviews with officials who were involved in the process to understand how the assignments were made in practice. We were told that the district officials had tried to place CHOs within the area of their residence but had not accounted for the exact distance between each subcenter and a CHO's home or for other subcenter characteristics. Whenever CHOs resided in the district headquarters or came from outside of the district, they were assigned across the entire district with the aim of achieving balance across subdistricts.[34]

[31] A second cohort of 500 CHOs was assigned in late December 2022. We include them in the control group throughout our analysis. This affects 14% of subcenters of our final control group sample.

[32] The reform was implemented while there was a large expansion in nursing school slots. Between 2017 and 2024, the annual number of slots for nursing students in Rajasthan increased from 14,650 to 21,350. In our survey data, we also find that most of the hired CHOs say that they would have otherwise worked in the public or private sector in urban areas. We should thus think of the reform as creating new public sector jobs in rural areas while the overall number of nurses in the state is expanding.

[33] To be eligible to receive a CHO, a subcenter first needed to be converted to a Health and Wellness Center. Such conversions mostly involved minor improvements to infrastructure. Subdistrict officials were responsible for nominating subcenters for conversion based on fixed set of criteria. See Appendix D for more details on the conversion process.

[34] District headquarters are located in urban centers and were not served by the Health and Wellness reform.

Data from an extended survey that we conducted with 243 Community Health Officers in Udaipur corroborate this process. Ninety-eight percent of the CHOs said that they were not involved in assignment decisions within the district. Figure A2 visualizes the assignment process through a map of subdistricts in Udaipur district. The bubbles in Panel A correspond to the previous residence of the CHOs. Forty-three percent of the CHOs came from the district headquarters and 12% came from outside the district. Panel B shows the locations of converted subcenters. To shed light on how the assignments were made, we present three examples in Panels C, D, and E. In Panel C, we observe that the five CHOs who previously resided in Kherwara subdistrict were all assigned to a subcenter near their previous residence in the southwestern part of Udaipur district. However, when comparing the assignments with the list of available subcenters in Panel B, we also see that the CHOs were not necessarily assigned to the subcenter that was located closest to their home. Instead, district officials relied on rules of thumb to make the assignments. Panels D and E further show that CHOs from the district headquarters or from other districts were assigned across all subdistricts. Finally, Panel F shows the final assignment outcomes in all of Udaipur district. We highlight that many subcenters in close vicinity differ in their treatment status, consistent with the idea that many assignments were based on ad-hoc decisions.

3.2.2 Matching and Estimation

Since assignments were based on CHO's preferences for districts and the location of their previous residence, subcenters that received a CHO were more likely to be located in less remote areas, making the parallel trend assumption less likely to hold in the unconditional sample. We address this concern by computing weights for the control group to match subcenters with and without a CHO based on our knowledge of the assignment rule. In particular, we use district fixed effects interacted with linear and squared terms of a facility's distance to the district and subdistrict headquarters to estimate propensity scores.[35] We then follow Abadie (2005) and use inverse probability weighting to adjust the control group. The intuition is that control group subcenters that were less (more) likely to have been assigned

[35] We do not use the distance to the nearest PHC since the name of the associated PHC was not included in the list of converted subcenters that was given to the district officials during the assignment process. The results are similar if we also use the distance to the PHC in the estimation of the propensity score.

a CHO receive less (more) weight, making the control group more similar to the treatment group.

In our preferred specification, we exclude districts in which more than 90% of subcenters received a CHO to ensure sufficient variation within districts and common support in propensity scores between treatment and control group units. We also exclude the subdistrict nearest to the district headquarters in each district since they were systematically more likely to receive treatment as most CHOs previously lived in the district headquarters.[36] Following the matching literature, we further implement common support restrictions by excluding observations within the top or bottom 2.5 percent of either the control or treatment group propensity score distribution in each district (Appendix Figure A3). In practice, this excludes the most and least remote subcenters from the sample. We show that our results are robust to alternative sample restrictions and matching strategies, including entropy balancing (Hainmueller, 2012) or estimating propensity scores based on LASSO regressions in Section 4.3.

Table 1 compares baseline covariates across treatment and control group subcenters in the unconditional and matched samples. Columns 1–4 show that treatment group subcenters are less remote, more literate, and cover a larger population than control group subcenters. However, once we reweight the control group based on the geographical information, we also achieve balance among most non-targeted subcenter characteristics (columns 5–8). The difference in the Scheduled Caste share remains significant but is small in magnitude.

We also note that any systematic variation in *levels* does not undermine the validity of the empirical design. Our primary identifying assumption is that in the absence of the new CHOs, control and treatment group subcenters would have followed the same *trends* in the outcomes of interest. This assumption would only be violated if treatment and control areas had differential trends in time-varying determinants of outcomes. For example, richer and less remote areas might have experienced a stronger decline in mortality outcomes during our sample period, even in the absence of the treatment. We thus focus our analysis on the reweighted sample that looks more similar on observables in the pre-period. We also use our

[36]Within a district, subcenters in the subdistrict nearest to the district headquarters were 15% (p-value = 0.000) more likely to receive a CHO than subcenters in other subdistricts.

empirical specification to check for differential pre-trends. The argument is that any changes between treatment and control group subcenters in the post-period are likely to be caused by the treatment if both types of subcenters followed similar trends before the CHOs were added. As we discuss below, we do not find evidence for pre-trends that would undermine our results.

We aggregate outcomes at the quarterly level since monthly data on deaths is very noisy. For subcenter i in quarter t, we estimate:

$$y_{it} = \alpha + \sum_{k=-8}^{k=-2} \beta_{pre}^{k} 1[D_{bt} = k] \times Treat_i + \sum_{k=0}^{k=7} \beta_{post}^{k} 1[D_{bt} = k] \times Treat_i + \delta_i + \eta_t + \epsilon_{it} \tag{8}$$

where $1[D_{bt} = k]$ is an indicator for k quarters between quarter t and the second quarter in 2022, the quarter the CHOs were added to the subcenters.[37] δ_i are subcenter fixed effects, which absorb any time-invariant factors like persistent facility characteristics such as infrastructure and local risks of diseases. η_t are quarter fixed effects that absorb common time trends such as seasonal variation in diseases. We cluster our standard errors at the subcenter level to account for serial correlation. To test for pre-trends, we report p-values for the null hypothesis that all pre-period coefficients are statistically equal to zero.[38]

We also run the standard difference-in-differences regression to analyze pooled treatment effects:

$$y_{it} = \alpha + \beta_1 Treat_i \times Post_t + \beta_2 Treat_i + \beta_3 Post_t + \delta_i + \eta_t + \epsilon_{it} \tag{9}$$

As a benchmark for the magnitude of the effects, we report the counterfactual treatment group means in the post-periods by subtracting the treatment coefficient from the observed treatment group mean in the post-periods (Basri et al., 2021). We top-code all continuous outcomes at the 99% level to reduce the influence of outliers.[39]

We replicate a similar empirical strategy for the analysis of our survey data. To maximize sample size, we include 71 subcenters that had a government-owned building but had not

[37] The assignment decisions were made at the end of March 2022, and the CHO started to work in the facilities in April 2022.

[38] We also note that, since our main analysis focuses on the first cohort of CHO assignments, all our treatment group subcenters were treated at the same time, so recent advances in staggered difference-in-difference methods are not applicable to our research design (Roth et al., 2023).

[39] We show robustness to alternative top-coding strategies in Appendix Table A13.

been converted to a Health and Wellness Center by March 2022. These subcenters were not eligible to receive a CHO when the assignment decisions were made, but they could have been eligible had the subdistrict officials chosen these subcenters for conversion first. We adjust the estimation of the propensity scores to be consistent with the criteria that were used to select facilities for conversion.[40] Appendix Table A3 shows that the reweighted survey sample is balanced.

3.3 First-Stage Results

We show in Table 2 how the CHO assignments affected subcenter inputs. Adding CHOs doubled the number of skilled workers (Column (1)) and increased the quality index by 0.68 standard deviations (p-value < 0.001, Column (2))). Figure 2 plots the checklist completion rate for the two medical vignettes for different providers in our endline survey and shows that the new CHOs perform better than ANMs and private providers but worse than PHC physicians in the adult asthma vignette.[41] Consistent with the ANM's existing focus on maternal and child healthcare services, we observe no differences in the checklist completion rate for the child dysentery vignette. Importantly, we also observe no effects in other subcenter characteristics, including the number of community health workers and the availability of equipment and medicines (Columns (3)–(6)).[42]

We next use data from time-use modules, unannounced visits, and patient exit surveys to provide suggestive evidence on how the CHOs affect subcenter performance. Since we only collected these survey components at endline, these results are not based on difference-in-differences regressions but instead exploit cross-sectional variation based on propensity score weighting.[43] Figure A4 presents the results from the time-use module and documents

[40]Subdistrict officials had to propose a fixed number of subcenters for conversion annually between 2018 and 2022. The minimum criterion for conversion was that the government must own the subcenter building. In some years, priority was further given to subcenters with electricity, running water, and good physical condition. To account for these criteria, we include baseline survey information on the condition of the subcenter building and the availability of electricity and running water in the estimation of the propensity scores for the survey sample.

[41]We can reject that the knowledge distributions of ANMs and CHOs are the same (p-value $= 0.007$). Similar knowledge distributions of treatment and control group ANMs suggests that there are no knowledge spillovers between the CHO and the ANM.

[42]These surveys were collected before part of the control group also received a CHO. Column (7) pools pre- and post-period means in the administrative data to show that using first-cohort assignments increases the likelihood of a CHO in a given post-period quarter by 85 percentage points.

[43]We use the same weights for the control group that we use for the matched difference-in-differences re-

that the CHOs primarily increase the time spent providing outpatient care and screening for chronic diseases. Adding CHOs to subcenters also made the opening hours of the facility more reliable. Using data from the unannounced audit visits, we find that having a CHO increases the likelihood that a subcenter is open at all on a given day from 42% to 69% (Figure 1). Treatment group subcenters are, on average, open for 2.8 more hours and see 51% more patients at the subcenter per day.

Using results from patient exit surveys, we also find that patients at CHO-staffed subcenters report higher levels of satisfaction (Appendix Table A5). In addition, patients at subcenters with CHOs were asked more questions and were more likely to have their blood pressure measured and be referred to a PHC.[44]

4 Results

We next use the administrative data to study the effect of CHOs on patient visits and mortality outcomes.

4.1 Effects on Patient Visits

The top left panel in Figure 3 shows the effects of the CHOs on patient visits over time.[45] While treatment and control group subcenters followed similar trends prior to the reform, we observe a substantial increase in the number of patient visits once the CHOs were added to treatment group subcenters. In Column (1) of Table 3, we show that the number of patient visits in a quarter increases on average by 216 visits (p-value < 0.001). Relative to a counterfactual treatment group mean of 371 patients per quarter, this represents an increase of 58%.

To examine substitution patterns and understand what patients would do in the absence of the reform, we use data from the CHIP household census on provider choices for 26,097

gressions. Since matched treatment and control group subcenter look very similar at baseline, substantial differences in subcenter performance at endline are likely related to the assignment of CHOs.

[44] These differences remain if we try to adjust for patient selection by controlling for symptom fixed effects or restricting the sample to patients who report having visited the subcenter before.

[45] Appendix Figures A5–A8 plot the trends in our main outcomes separately for treatment and control group subcenters and show that the treatment effects are driven by a trend break in treatment group units. Modest improvements in control group outcomes over time can be attributed to control group subcenters that received a CHO in subsequent quarters.

household members who suffered from at least one symptom in the last 30 days across Rajasthan. We find that the share of households who visited the subcenter increased from 34% to 47% in treated areas (Figure 4). We further observe an equivalent decline in the share of respondents who report not seeking any healthcare when sick. These patterns indicate that patients do not substitute away from other existing healthcare facilities but that the CHOs manage to reach patients who were outside of the healthcare system before the reform.

Using data from the PCTS portal, we also examine the types of patients that visit the subcenter. The increase in patient visits does not only include the treatment of minor symptoms like mild coughs and fever, but also the diagnosis and treatment of potentially life-threatening conditions. Column (2) in Table 3 shows that the number of patients with acute heart diseases (like heart attacks) increases by 67%. While the average subcenter only treats 0.036 acute heart disease patients per quarter, providing medical support to such patients could lead to immediate effects on mortality outcomes. CHOs would not be able to perform surgeries, but could, for example, provide aspirin to thin the blood and improve blood flow and then refer patients to higher-level facilities. We also observe increases in the number of epilepsy but not stroke patients (Columns (3)-(4)). From April 2023 onwards, the administrative data further provide additional details on the types of patients that visit subcenters. When comparing post-period means between treatment and control group subcenters in Appendix Table A6, we do not only see increases in patients with general eye and oral diseases, but also in patients with serious conditions, including chronic obstructive pulmonary disease (COPD) and asthma.

In addition to affecting acute healthcare services, the CHOs also improved the provision of preventive healthcare services by screening patients for chronic diseases (Columns (5)-(6)). The number of hypertension patient visits increases by 72% (p-value < 0.001), and the number of diabetes patients increases by 62% (p-value < 0.001). As mentioned in Section 3.1, these outcomes include newly and previously diagnosed patients. While the increase in patients diagnosed with hypertension and diabetes could theoretically also be driven by an increase in the prevalence of chronic diseases, low awareness rates in the population make it much more likely that the increase in chronic disease patients is due to higher screening rates.[46] We use data from the Health and Wellness Center portal that is only available for treatment group subcenters to get a better understanding of how CHOs affect chronic patients. Appendix Figure A9 shows a clear increase in the number of hypertension and diabetes patients who are screened, newly diagnosed, or treated after the CHOs were added to subcenters.

We also examine the effect of CHOs on an index of five maternal and child health services. The direction of the treatment effect is ambiguous ex ante. Even if CHOs only had limited involvement in maternal and child health services, their presence could still have freed up additional capacity for the ANMs. However, an increased focus on chronic diseases and basic outpatient care could have also led to a neglect of existing services. Overall, we do not observe substantial changes in the provision of maternal and child health services. Appendix Table A7 shows that we also find no improvements when we analyze each index component individually.

4.2 Effects on Mortality Outcomes

We study the effects of CHOs on mortality outcomes by using statewide PCTS portal data on deaths by age group in each subcenter catchment area. In our preferred outcome specification, we examine a binary version of whether a subcenter reports any death in a particular quarter.[47] Panel A in Figure 5 shows the event-study graph for whether any death was reported in the subcenter area. Except for an outlier in the third pre-quarter, we find that the coefficients for the seven pretreatment quarters are neither individually nor jointly significant (p-value = 0.150). However, once the CHOs are added to treated subcenters, we observe a significant decline in the likelihood that any death occurred in the catchment area in a particular quarter for seven out of the eight observed quarters. When examining the effect by age group, we observe that the effect is largely driven by a decline in whether there was an elderly death in the catchment area (Panel B).[48]

While the reform did not explicitly target the elderly, it is not surprising that the effect

[46] Only 37% of hypertensive patients in India are aware of their condition (Amarchand et al., 2022).

[47] The benefit of the binary outcome is that it is less noisy than examining mortality rates. On average, 36% of subcenters report at least one death in a given quarter. Conditional on reporting any death, 34% report one death, 22% report two deaths, and 15% report three deaths.

[48] Appendix Figure A10 shows that we find no effects for other age groups.

is concentrated among this age group since their health outcomes are most likely to be affected by improved access to public primary healthcare services. Data from the 2017–2018 National Sample Survey (NSS) show that 56% of total deaths occur among the elderly, making it more difficult to be able to observe changes in mortality for other age groups (see Appendix Figure A11 for a distribution of age at death). Appendix Figure A12 further shows the distribution of common causes of death by age group. Younger adults mainly die from injuries that will likely require trauma care services that are only provided by higher-level facilities. By contrast, most of the elderly die due to cardiovascular diseases which could have been prevented by earlier diagnosis and treatment options that are available at subcenters.

Table 4 reports aggregate effects by pooling all pre- and post-periods. In addition to the binary outcome, we also report effects on the total number of deaths, mortality rates in levels, and the inverse hyperbolic sine of the mortality rates.[49] We observe significant declines for all of these outcomes, including a 10% decline in all-age mortality rates (p-value = 0.035) in Column (3) of Panel A. Eighty percent of the decline in all-age deaths is attributable to the reduction in elderly deaths (Panel B). We observe no significant effects in the mortality outcomes of other age groups (Panel C).[50] Appendix Table A9 further breaks down the elderly deaths into different causes of death. We observe that the aforementioned decline in deaths can be completely attributed to a decline in deaths from unknown causes, a category which covers 59% of all reported deaths. While the high rate of unknown-cause deaths is a limitation of our data, the decline in this category is also consistent with earlier diagnosis rates for acute and chronic diseases contributing to the observed effects.

A potential concern is that differences in reporting could explain our results. The implied annualized elderly mortality rate of 10.6 deaths per 1,000 elderly individuals in the administrative data is only around 45% of the elderly mortality rate of 20.6 deaths per 1,000 elderly individuals observed in the NSS 2017–2018 survey, suggesting that many deaths remain unreported. However, as mentioned in Section 3.1, the ANM remains the person who fills out the information forms for the PCTS portal. In our time-use module, we also do not find

[49] The mortality rate is defined as the number of deaths per 1,000 individuals. When reporting results separately by age group, we multiply the total population in the catchment area by the average population share of this age group in the Socio Economic Caste Census in 2011.

[50] Appendix Table A8 splits Panel C into four age groups. We also find no effects when we analyze infant, child, adolescent, and adult mortality, separately.

that ANMs at treatment group subcenters spent more days on administrative and reporting tasks (Appendix Figure Figure A4). Better reporting would likely also go against us finding declines in mortality since an increase in the quality of reporting should increase the reported number of deaths in treatment areas. A general increase in the quality of reporting should further show up in other outcomes as well, including maternal and child health indicators.

Qualitative surveys with ANMs suggest that one reason why deaths are underreported in the administrative data is that some of them think that they only need to fill in the maternal and child health indicators and leave the adult and elderly death forms blank. In Appendix Table A10, we thus restrict the sample to subcenters that reported at least one elderly death in the pre-periods, a sample for which the control group means are more similar to the elderly mortality rates in the NSS 2017–2018 survey (21.9 relative to 20.6 deaths per 1,000 elderly individuals). Reassuringly we still find significant declines in the mortality outcomes for this subgroup. We also observe no decline in the likelihood of observing any elderly death in the post-period at all, addressing concerns that the CHOs might have encouraged ANMs to stop reporting such deaths completely.

As an additional check, we obtained access to another administrative dataset, the Civil Registration System, that separately maintains death records at the gram panchayat level. Since some gram panchayats have more than one subcenter in their area, we redefine the treatment assignment to indicate whether at least half of the subcenters in the gram panchayat received a CHO. While this leads to noisier results, we still observe a significant decline in elderly deaths in this dataset (Column (5) in Appendix Table A10).

Finally, we also examine effects on patient outcomes using data from the two rounds of household surveys we conducted. Since our household survey sample is too small to detect changes in mortality outcomes, we instead focus on the incidence of health symptoms and medical spending in the past 30 days as well as hospitalizations in the past six months. While we observe no effects on the incidence of symptoms (Column (1)) and overall medical spending (Column (2)), we find a decline in hospitalizations by 1.7 percentage points (p-value = 0.030, Column (3)). Consistent with previous results, this effect is also driven by a decline in hospitalizations among the elderly (Appendix Table A11).

4.3 Robustness

We implement various robustness checks for our results. We find similar treatment effects if we use alternative common support restrictions (Appendix Table A12) and top-coding strategies (Appendix Table A13). A potential concern is that the district officials also used additional information besides the geographical location of the subcenter to assign CHOs. Panel A in Appendix Table A14 estimates propensity scores using a LASSO regression based on all variables listed in Table 1 instead. Even if district officials only used geographical information, it is also possible that the functional form of the propensity score function is incorrectly specified. We address this concern by replicating our analysis with entropy balancing weights (Hainmueller, 2012). This method chooses the set of control group weights that minimally deviate from uniform weights while matching a specific set of moments between the treatment and control groups.[51] The decline in all-age mortality outcomes becomes insignificant with entropy balancing, but we find similar effects on elderly mortality outcomes in both instances. As shown in Appendix Table A15, our results on elderly mortality are also robust to including subcenter-specific linear time trends in the regression and using the double-robust difference-in-differences estimator proposed by Sant'Anna and Zhao (2020). Our results are further robust to removing the Covid-affected quarters from our sample period or accounting for the 14% of control group subcenters that also received a CHO at a later date.[52]

Another concern relates to spillover effects. The average control group subcenter is 6.6 kilometers away from the nearest treatment group subcenter. In practice, we do not observe that any household members in our survey data ever visit a different subcenter besides the one in their catchment area. The likely reason is that patients prefer to go directly to the nearest physician-staffed public clinic (PHC), which is, on average, 7.3 kilometers away. We further note that the existence of spillover effects would likely lead us to underestimate the treatment effects since control group households would also experience the benefits of improved access to health care.

[51] Following our information on the assignment rules, we use the average distance to the district and subdistrict headquarters and the average share of subcenters in each district as our matching moments.

[52] Appendix Table A3 also shows that the number of Covid-19 cases and deaths at baseline in the survey sample does not predict CHO assignments.

We also conduct an alternative empirical strategy in which we match each treatment group subcenter to the closest control group subcenter.[53] The intuition is that subcenters in the same geographical vicinity should follow the same trends in health outcomes in the absence of the CHOs. Appendix Table A16 shows that we find similar treatment effects if we use this approach instead.

5 Mechanisms & Counterfactual Policies

In this section, we first examine which mechanisms can explain the decline in mortality rates. We start by showing reduced-form evidence based on heterogeneity analysis and private provider surveys. We then combine the administrative and survey data to estimate a discrete choice model of patient demand to quantify the impact of each channel and evaluate counterfactual policies.

5.1 Reduced-Form Evidence

Our conceptual framework suggests that three channels might have contributed to the decline in mortality outcomes: (i) better access to subcenter services, (ii) higher provider quality at subcenters, and (iii) changes to private sector behavior.

5.1.1 Improvements in Access vs. Quality

How important are the first two channels? While our policy variation does not allow us to directly distinguish between them using reduced-form evidence, we take a step in that direction by analyzing whether the treatment effects vary based on the quality difference between the ANM and the CHO. For that, we generate a dummy variable for whether the difference in the quality index between endline and baseline in treated areas is equal to or larger than the median difference. Consistent with improving quality being an important channel, we observe larger declines in mortality outcomes in catchment areas for which quality has increased more (Table 6, Columns (1)–(5)).[54] However, we do not observe a

[53] We implement matching with replacement, allowing each treatment group subcenter to be matched to more than one control group subcenter.

[54] Appendix Table A17 repeats this analysis by instead splitting the sample separately by the CHO quality index and the ANM baseline quality index. While the results are weaker in these specifications, we still find suggestive evidence that the health effects tend to be larger for higher-quality CHOs and smaller for subcenters that already had a high-quality ANM at baseline.

larger increase in patient visits for this subgroup, indicating that patient choices do not seem to be sensitive to quality changes. In a similar exercise, we also split the sample based on the increase in subcenter person-hours in Columns (5)–(8).[55] Besides the addition of the CHOs, subcenter person-hours vary based on whether an ANM lives in a village and whether an ANM has to take care of two subcenters at the same time due to vacancies. In this heterogeneity analysis, we find that a larger increase in subcenter person-hours also leads to a larger increase in patient visits but not to more improvements in health outcomes. Taken together, these findings suggest that the increase in healthcare quality seems to be important for reducing mortality but that improvements to healthcare access are necessary to also increase healthcare utilization. We explore this interaction in more detail in our counterfactual simulations in Section 5.2.

5.1.2 Effects on Private Provider Behavior

We next investigate how the private sector responded to the reform. A recent set of studies has documented the existence of multiplier effects in education, where private schools react to increased competition from public schools by increasing their quality (Andrabi et al., 2024; Dinerstein et al., 2023).[56] However, other work has also shown that increased competition in healthcare markets could increase the adoption of potentially harmful practices that are demanded by patients, including the overuse of antibiotics and opioids (Bennett et al., 2015; Currie et al., 2023). More broadly, increased competition could also hurt some patients by leading to private provider exit (Dinerstein and Smith, 2021) or higher private sector prices through market segmentation (Atal et al., 2024).

We find no treatment effects on the total number of providers in the market (Column (1), Table 7).[57] Instead, we thus focus on analyzing treatment effects on provider attributes in Columns (2)–(5). While we do not find evidence for changes in the number of patients, prices, or working hours, (Columns (2)–(4)), we observe that adding CHOs to subcenters increases the quality index of private providers by 0.29 standard deviations (p-value = 0.034, Column

[55] Appendix Figure A13 shows the distribution of the difference in the quality index and person-hours between baseline and endline.

[56] In healthcare, Bennett and Yin (2019) further show that the introduction of chain pharmacies in India improved drug quality among incumbents.

[57] We also observe no significant changes if we separately examine treatment effects on entry and exit.

(5)).[58] Consistent with a decline in local market power, these improvements are concentrated among providers that were the only private providers in the catchment area at baseline (Panel B).[59] We observe no differences in the use of antibiotics or injections in medical vignettes or when we ask providers about the share of patients that received antibiotics or injections in the past 30 days (Appendix Table A19), providing evidence against concerns that potentially harmful behavior could increase with competition (Bennett et al., 2015).

Overall, these results suggest that adding CHOs to subcenters also improved private sector quality, potentially multiplying the effect on patient outcomes. More broadly, our findings also contribute to the debate on the role of patient demand and provider financial incentives (Currie et al., 2014; Lopez et al., 2022) by providing suggestive evidence that local market power leads private providers to underinvest in quality.

5.2 Demand Model

To quantify the effect of each channel in more detail, including the importance of the private sector responses, we require additional structure. In the remaining part of this section, we thus estimate a structural model of patient demand that allows us to use counterfactual simulations to shut down each of the three channels separately. The results decompose the effects and also directly inform optimal staffing policies, including, for example, whether it would have been better to assign a second ANM instead of a more highly qualified CHO, a variation of the policy that has been implemented in the state of Tamil Nadu (Muraleedharan et al., 2018). The model further allows us to predict whether CHOs have differential effects for poor and non-poor patients. We also evaluate the effect of a ban on private providers to document the value of the private sector before and after the reform.[60] Finally, we examine

[58] Effects on the separate quality index components are reported in Appendix Table A18. Treatment effects on the length of the medical degree are driven by a 13 percentage point increase in the share of private providers that are currently enrolled in a degree program (p-value = 0.064). Whenever a provider is enrolled in a degree program at the point of the survey, we use the expected length of the medical degree based on the assumption that the provider will finish the current program.

[59] In Columns (4) and (5) in Appendix Table A18, we also examine whether we observe differential treatment effects on mortality outcomes based on the number of private providers at baseline. While we only find large negative coefficients for areas that only have one private provider, our sample is too small to reject that the treatment effects are the same.

[60] Carneiro et al. (2024) evaluate a similar counterfactual to estimate the value of private schools in Pakistan. While a ban on private providers would mechanically reduce patient welfare in the demand model, the predicted effects on average quality and all-age mortality are ambiguous since the direction of the effect depends on two countervailing forces. Some patients would benefit since they would start going to

how the marginal effect of CHOs differs across locations and how much mortality could have declined if assignments had taken local market conditions into account.

5.2.1 Mapping the Model to the Data

We expand the conceptual framework described in Section 2.4 to take the model to the data. We use the 46 PHC catchment areas in our survey sample to define markets.[61] Each market is observed at baseline and endline. The sizes of locations in a market are based on population shares in the 2011 population census and imputed poverty shares for each location are retrieved from the SHRUG data. We assume that 20% of the population is suffering from at least one symptom in a given month.[62] We further assume that patients can only choose one provider, that there are no referrals, and that providers do not face capacity constraints.[63]

We combine our administrative and survey data on patient visits for public and private providers to create market shares. Provider characteristics are obtained from survey data. The main provider characteristics are their location, person-hours (h_{jt}), quality (q_{jt}), and price (p_{jt}). We use our quality index to measure quality (q_{jt}) and define person-hours (h_{jt}) as the sum of total hours worked by all healthcare workers in a facility in a typical week.[64] Prices (p_{jt}) for private providers are obtained by asking them about their typical fee, including medicine and consultation fees.

Additional provider characteristics (x_{jt}) include an infrastructure index, a medicine index, the provider's years of experience, the number of years the provider is working in the village, and dummy variables for provider type (subcenter, PHC, or private). We also include a

better public providers, but other patients would be worse off since they would not seek healthcare at all (Godlonton and Okeke, 2016).

[61] Patients rarely visit a provider outside of the PHC catchment area for outpatient care. In the household census data, only 4% of patients in our sample area report visiting another public provider besides the subcenter and PHC or visiting a private provider in another town.

[62] This assumption is informed by our own data and nationally representative household surveys. For example, 20% of rural households in the Indian Human Development Survey 2012 report being sick for at least one day in the past month. We also assume that the likelihood of getting sick is the same for poor and non-poor patients. This is supported by our survey data, where we find that, within the same village, the share of household members who had any symptoms in the past 30 days does not differ by poverty status.

[63] Abstracting from capacity constraints is reasonable in our context since the average provider only treats 2.3 patients per person-hour, indicating that most providers have excess capacity. Less than one percent of providers treat more than 20 patients per person-hour.

[64] We normalize the quality index such that the index is zero is all of its components are zero.

provider-specific term that affects a patient's utility but is unobservable to the econometrician (ξ_{jt}). This term includes attributes like the provider's attitude towards patients. We allow patient preferences over distance, quality, and price to differ by poverty status. We further include the vector ν_i to allow preferences for prices to be heterogeneous across an unobserved patient characteristic. Patients also have random preference shocks for providers (ϵ_{ij}).

The revised utility function is

$$u_{ijt} = \beta_i^q q_{jt} + \beta^h h_{jt} - \alpha_i p_{jt} - \lambda_i d_{ij} + \beta x_{jt} + \xi_{jt} + \epsilon_{ijt}, \tag{10}$$

with $\beta_i^q = \bar{\beta}^q + \beta_1^q poor_i$, $\alpha_i = \bar{\alpha} + \alpha_1 poor_i + \nu_i$, $\nu_i \sim^{iid} \mathcal{N}(0, \sigma^2)$, and $\lambda_i = \bar{\lambda} + \lambda_1 poor_i$, where $poor_i$ is an indicator variable for whether the patient comes from a poor household.[65] Based on these changes, the share of non-poor patients who live in location l in period t and select provider j becomes

$$s^l_{j,t,nonpoor}(\mathbf{q}, \mathbf{p}, \mathbf{h}, \theta) = \int_\nu \left(\frac{exp(\bar{\beta}^q q_{jt} + \beta^h h_{jt} - \bar{\alpha} p_{jt} - \nu p_{jt} - \bar{\lambda} d_{ij} + \beta x_{jt} + \xi_{jt})}{\sum_n^{J_t^m} exp(\bar{\beta}^q q_{nt} + \beta^h h_{nt} - \bar{\alpha} p_{nt} - \nu p_{nt} - \bar{\lambda} d_{nl} + \beta x_{nt} + \xi_{nt}) + 1} \right) d\nu. \tag{11}$$

For modeling supply-side responses, we take a reduced-form approach by using our estimated treatment effects on the private provider quality index. We use separate treatment effects based on the number of private providers in a specific location to proxy for differences in the baseline market power of private providers based on estimates from Column (5) in Panel B in Table 7.[66]

To translate predicted choice outcomes into mortality effects, we assume that there is a structural relationship π between the elderly mortality rate and the unconditional mean

[65] We focus on modeling heterogeneity in patient preferences across three provider characteristics since we use three micro-moments from the household census data. We found little differences in patient preferences for person-hours when estimating an extended model with additional interaction terms.

[66] An alternative approach would be to use the first-order conditions of private providers to back out marginal costs and simulate equilibrium responses. While this would allow for a richer model of private sector behavior based on changes in local market power, it would require strong assumptions on the profit maximization behavior of private providers who might also have an altruistic motive. Since one of the primary objectives of our model is to decompose mechanisms, we consider the reduced-form approach to be sufficient for our setting.

quality in each location, where we define unconditional mean quality as the average weighted quality of care received in the location, taking $q = 0$ if the patient chooses the outside option. We then recover π in two steps. First, we use the CHIP data to calculate the difference in unconditional mean quality between treatment and control areas across the entire state. Second, we use our difference-in-differences estimates for the treatment effects on all-age mortality. The combined estimates imply that an increase in unconditional mean quality by 0.282 standard deviations results in a 10% decline in all-age mortality.

5.2.2 Estimation and Identification

We follow Berry et al. (2004) and estimate the demand parameters using simulated methods of moments. The aggregate moments match the model's market share prediction for each provider ($s_{j,t}(\theta)$) to those in the data ($s_{j,t}$):

$$s_{j,t} - s_{j,t}(\theta) = 0 \tag{12}$$

Since providers might strategically choose their quality, person-hours, and prices, we need an instrument for these provider characteristics.[67] To construct instruments, we use CHO assignments and a dummy variable for whether the healthcare workers in public facilities live in the village to generate exogenous variation in quality and person-hours. We further use survey data on the source of medicines for each private provider based on the idea that variations in medicine costs across supplier locations are not correlated with the unobserved provider term ξ_{jt}.[68] Appendix Table A20 shows the first-stage results. For each instrument (z_{jt}), we define a second set of moments based on the set of orthogonality conditions:

$$\mathbb{E}[\xi_{jt}(\theta) z_{jt}] = 0 \tag{13}$$

For the micro-moments, we use information on individual-level choices for 771 household members in the post-period from the CHIP household census data. We construct an asset

[67] For example, private providers with better attitudes towards patients, captured by the unobserved provider term ξ_{jt}, might be able to charge more, which would lead us to underestimate how much patients dislike prices.

[68] Appendix Table A22 shows that we find similar demand estimates if we instead use a Hausman-syle instrument based on average private provider prices in neighboring markets.

index to classify households in the CHIP data as poor or non-poor.[69] We ask the model to match the poverty share among patients who visit the subcenter, PHC, and private providers in the CHIP data as well as the share of patients who live in the PHC location among patients who visit the PHC:

$$\mathbb{E}[poor_i|\{i \text{ chooses a subcenter}\}] = 0 \tag{14}$$

$$\mathbb{E}[poor_i|\{i \text{ chooses a PHC}\}] = 0 \tag{15}$$

$$\mathbb{E}[poor_i|\{i \text{ chooses a private provider}\}] = 0 \tag{16}$$

$$\mathbb{E}[lives\ in\ PHC\ location_i|\{i \text{ chooses a PHC}\}] = 0 \tag{17}$$

Finally, we use the covariance in provider prices between the first- and second-order choices in our household survey data:[70]

$$\mathbb{C}(p_j, p_{k(-j)}|j, k \neq 0) \tag{18}$$

The intuition behind the estimation strategy is that we will find a vector of parameters that match the observed and predicted aggregate market shares for each provider, while also trying to meet the orthogonality conditions, the average patient type for each provider category, and the covariance in prices between first- and second-order choices. Using the nested fixed point algorithm, we recover mean utilities for each guess of the non-linear parameters. We can then recover the linear preference parameters by running an IV regression of the recovered mean utilities against the provider characteristics. Estimates are obtained using the optimal two-step weighting matrix.

Identification is based on multiple sources of variation. We assume that cost differences across supplier locations affect the pricing decision of private providers and that a private provider's choice of supplier location is not correlated with unobserved provider attributes. We already showed in our previous analysis that the assignment of CHOs generated variation in person-hours and quality at subcenters. In addition, person-hours at public facilities vary

[69] The resulting village-level poverty shares in the CHIP data are closely correlated with the imputed poverty shares in the SHRUG data (Appendix Figure A14).

[70] We asked respondents to which provider would they go first if they were to suffer from a mild or moderate symptom (like a cough or mild fever) and to which provider they would go if their first choice was closed.

based on whether the healthcare workers live in a village, a decision which is often related to personal circumstances, including whether they have children of school-going age (Mohan et al., 2003). To support the argument that variation in person-hours is not related to other public facility attributes, we show in Appendix Table A21 that catchment area characteristics do not significantly predict whether an ANM lives in the subcenter village at baseline.[71] We further take the number of public and private providers across locations as exogenous, which allows us to compare differences in choices depending on provider availability.

Our micro-moments help to identify how preferences differ by poverty status and location. Information on second choices further pin down the unobserved preference heterogeneity since, given mean choice probabilities, a higher variance of ν_i should lead to a higher covariance in prices across first and second choices (Berry et al., 2004).

5.2.3 Estimates

We report the estimated demand parameters in Table 8. As we would expect, patients like person-hours and dislike prices and distance. We also find that poor patients tend to be more sensitive to prices, but the difference is noisy and not significant. The implied magnitudes of the estimates appear reasonable, with poor patients being indifferent between paying an additional INR 100 (USD 1.20) or traveling two kilometers more. The estimates also suggest that patients undervalue provider quality given the observed declines in mortality outcomes, as poor patients would only be willing to pay INR 46 (USD 0.54) more to visit a provider whose quality index is one standard deviation higher. As we observe that an increase in unconditional mean quality by 0.282 points is associated with a 10% decline in all-age mortality rates, the preference estimate suggests that patients do not fully internalize the benefits of visiting a higher-quality provider.[72]

We next assess model fit. Appendix Table A23 shows that the model fits the data well, as the poverty shares among patients who visit subcenters, PHCs, and private providers in the model are very similar to what we observe in the data. We further assess the perfor-

[71] Another concern is that the living arrangements of public healthcare workers affect other parts of service provision. However, we do not that proxies of provider performance are correlated with whether a public healthcare worker lives in the village.

[72] This could be because patients underestimate the returns to visiting a high-quality provider or because they only observe a noisy signal of quality.

mance of the model by plotting changes in subcenter market shares between our baseline and endline surveys in the treatment group against the treatment effects predicted by our model. While the data is noisy, we find that the predicted and observed values tend to be strongly correlated, suggesting that the model is doing well in capturing patient choices. As an out-of-sample test, we also plot our estimates of the unobserved provider term ξ_{jt} against a subjective assessment of providers by our surveyors that is not included in the model. Appendix Table A16 shows that we find a significantly positive relationship between both variables, further increasing our confidence in the model results.

5.2.4 Counterfactuals

We start by considering four counterfactuals in which we separately change subcenter person-hours, subcenter quality, and private provider quality to decompose the effects. In these counterfactuals, we treat all subcenters in our sample area and compare changes to outcomes relative to the baseline scenario in which only one ANM is working at each subcenter. The first counterfactual models the full treatment effects of the CHO assignments, in which CHOs working alongside the existing ANMs, leading to an increase in subcenter quality and subcenter person-hours as well as an increase in private provider quality. In the second counterfactual, we evaluate what would happen if we only increased subcenter quality, akin to a policy in which we would replace the existing ANMs with the new CHOs instead of adding CHOs alongside the ANMs, while holding private provider quality fixed. In the third counterfactual, we only increase subcenter person-hours, which would be equivalent to a reform that would assign a second ANM instead of a more qualified CHO. The fourth counterfactual accounts for the increases in subcenter quality and subcenter person-hours but assumes that private providers did not change in response to the reform.

Table 9 starts by showing the baseline scenario without CHOs being added to any subcenter (Row (1)). In this case, the average subcenter market share is 10% (Column (1)). This goes up to 26.5% if we evaluate the full treatment effect in which CHOs are added to all subcenters and private providers respond by increasing their quality (Row (2)). Only improving subcenter quality or person-hours would increase their market shares to 16.4% and 18.2%, respectively. Had private providers kept their initial quality, subcenter market shares would have gone up to 26.8% due to the CHOs.

Column (4) reports changes in average quality and Column (5) reports predicted changes in all-age mortality rates. We find that the full treatment increases the average quality index of the chosen healthcare providers by 0.291 standard deviations, leading to a decline in all-age mortality by 10.3%. Improving only subcenter quality or subcenter person-hours would achieve approximately 33% and 23% of the observed effect, respectively. The reason why solely improving subcenter quality has a relatively small effect is that many patients do not value quality highly. By contrast, patients value increased access, but infra-marginal patients who would have visited the subcenter anyway do not benefit from increased person-hours. Row (5) shows that, if subcenter quality and person-hours are improved simultaneously but private providers do not change their quality, all-age mortality would decline by 9.3%, suggesting that 10% of the decline in all-age mortality rates can be attributed to private sector responses.

We evaluate the ban on private providers in Rows (6) and (7). We find that average mortality would slightly increase in these cases as worse health outcomes for patients who would stop seeking healthcare altogether dominate improvements in health outcomes for patients who would instead visit a subcenter or PHC. The negative effect of the ban on private providers holds even after the arrival of CHOs, suggesting that private providers continue to play a relevant role in the healthcare sector.

Adding CHOs to subcenters has a large effect on health outcomes on average, but there is substantial heterogeneity in the marginal effect of adding a CHO to individual subcenters. In the top plot in Figure 6, we rank subcenters according to how adding a CHO would change mortality rates based on counterfactual simulations. For 3% of subcenters, we even find that adding a CHO would lead to worse health outcomes since the substitution effect from patients switching away from higher quality providers would dominate. These findings already suggest that the current policy in which all subcenters are supposed to receive a CHO at some point is not optimal.

Determining the optimal number of CHOs without further information requires strong assumptions related to government budget constraints and the value the government assigns to life years saved. Instead, we investigate how much an optimal reallocation of providers could improve health outcomes given a fixed number of CHOs. To do that, we consider the first cohort of CHOs that the government had assigned to our sample area in March 2022.[73] We first find in Rows (9) and (10) that the observed government allocation only performs slightly better than what we would predict under random assignment (either within or across markets).[74] We also evaluate five rule-based assignment schemes that prioritize (i) subcenters with the highest poverty rates, (ii) subcenters that only had one private provider in the catchment area at baseline, (iii) subcenters with the largest catchment population, (iv) subcenters that are furthest away PHCs, and (v) subcenters with the lowest baseline quality. The best-performing assignment rule uses baseline subcenter quality and increases the mortality effect by 15%. Finally, we also examine the optimal assignment of CHOs using the full information in the model. We estimate that the optimal allocation would CHOs across all subcenters would lead to a 33% greater decline in mortality outcomes. Appendix Table A24 shows that the optimal assignment achieves these gains by prioritizing subcenters that are further away from PHCs, have lower baseline quality, have a larger catchment population, and are located near private providers with market power. However, a concern is that such assignments might not be politically feasible since CHOs have strong preferences to be located close to their homes. We thus also evaluate gains from reallocating CHOs within the same markets to which they are already assigned based on the assumption that CHOs are mostly indifferent between being assigned to different locations within the same geographic area. Using the within-market restriction we find that reallocating CHOs could improve the mortality effects by 18%.

So far, we focused on the average impact of the reform. By estimating different preferences for poor and non-poor patients, we can also assess how the different counterfactuals affect health equity. Appendix Table A25 shows assigning CHOs to all subcenters would lead to a larger effect for poor households. Poor patients mostly benefit from the direct improvements in public healthcare provision, while the indirect effects on the private sector play a more important role for non-poor patients. When considering the optimal allocation of CHOs, the

[73] Fifty-one percent of subcenters in our sample area received a CHO in March 2022. In all simulations, we assume that all CHOs have the same quality and lead to the same increase in person-hours.

[74] A reason for the better performance of the observed government allocation is that the government, by prioritizing less remote clinics, allocated CHOs to clinics that serve a larger catchment population.

government might also put more weight on supporting poor households to prioritize health equity. In Figure 7, we demonstrate the trade-off that governments face by showing how the optimal allocation of CHOs changes effects for poor and non-poor patients depending on how much weight is assigned to each group. If the government only cares about maximizing the average impact, it could achieve an average mortality decline by 8% and a mortality decline for poor households by 8.5%. By contrast, if the government only cares about maximizing the impact for poor households, it could achieve an average mortality decline by 7.7% and a mortality decline for poor households by 8.8%.

6 Discussion

In the final part of the paper, we discuss the plausibility of the size of the treatment effects, alternative mechanisms, and the cost-effectiveness of the reform.

6.1 Plausibility of Treatment Effects

An important question is whether the size of the effects is reasonable. The relevance of better access to basic healthcare services for improving mortality outcomes is supported by NSS 2017–2018 survey data: nearly 40% of deceased adults did not receive any medical attention before their death, suggesting that even small changes to healthcare services could lead to substantial improvements in health outcomes. Another way to address the concern is to look at previous studies that examined the effect of improved public healthcare on short-run health outcomes. Past research has shown that adding a physician to a public facility or improving community monitoring can reduce infant and child mortality by 20%–33% within one year of an intervention (Björkman and Svensson, 2009; Okeke, 2023).

Studies on infant and child mortality, however, might be less informative since health outcomes of new infants are likely to be more sensitive to targeted improvements to public healthcare than the health outcomes of the elderly. While there is less evidence on the short-run effects of public health interventions on adult mortality, existing work also documents the potential for declines in adult mortality outcomes within a short time frame.[75] Bailey

[75]Previous studies also examine the effect of non-medical interventions like pension payments and cash transfers on mortality outcomes and find mixed results (Barham and Rowberry, 2013; Huang and Zhang, 2021; Malavasi and Ye, 2024; Jensen and Richter, 2004; Snyder and Evans, 2006). Among those who find

and Goodman-Bacon (2015) focus on the long-run effects of community health centers in the US, but their event study graphs suggest that adult mortality already declined sharply within the first year of treatment. Besides improvements to primary care, previous work has shown how increased access to healthcare insurance can lead to a decline in adult mortality. Gruber et al. (2023) report that a healthcare insurance expansion in China decreased adult mortality by 12%, with some of these effects already occurring within the first year. Sood et al. (2014) further find that healthcare insurance in India reduced mortality from conditions covered by the scheme by 64% within two years.[76]

The size of the treatment effects also appears large due to the definition of the mortality outcome. To illustrate this, consider an intervention that would increase life expectancy for everyone in the treatment group by two years. In that case, the number of observed deaths within two years after the intervention would decline by 100% since nobody would die in the treatment group during the observed time frame. However, in the following years, the individuals who initially survived would start to die and the number of deaths in each period between the treatment and control groups would be the same (assuming that all cohort sizes are the same). Following a similar intuition, a CHO-induced increase in elderly life expectancy by 3 months would be sufficient to generate the decline in elderly mortality rates that we observe in the administrative data. Alternatively, we can also estimate a Gombertz mortality model using age-specific mortality rates from the NSS 2017–2018 survey. The model consists of a base mortality rate and a growth rate of mortality with age. Assuming that the reform decreased the base mortality rate by 10%, we would predict an increase in elderly life expectancy by 16 months.

The effects of CHOs on mortality outcomes could either be attributed to higher screening rates for chronic diseases or better management of outpatient care patients. In practice, both of these services are interlinked, since many patients who visit subcenters for outpatient

positive effects, Barham and Rowberry (2013) show that a conditional cash transfer in Mexico reduced municipal-level elderly (65+ years) mortality rates by 4% and Huang and Zhang (2021) show that a pension program in China reduced mortality rates by 12%.

[76]Existing research on the effect of chronic disease screenings on mortality outcomes also mostly focuses on longer-run outcomes. Among studies that find short-run effects, Lin et al. (2004) show that a hypertension mass campaign led to a substantial decline in stroke mortality within a year in Taiwan, and Hickey et al. (2021) find that patient-centered hypertension care reduced all-cause mortality among adults with uncontrolled hypertension by 21% within three years in rural Kenya and Uganda.

care are automatically checked for common chronic diseases like hypertension, a practice known as 'opportunistic screening'. While our data does not allow us to directly identify which services prevented specific deaths, we can use our estimates to get a general sense of their relative importance. A back-of-the-envelope calculation that combines the treatment effects on hypertension and diabetes patients with estimates from the medical literature and nationally representative household surveys suggests that around 13% of the reduction in all-age mortality can be associated with better screening for chronic diseases (see Appendix E for details). The increase in acute heart disease and epilepsy patients could together explain 35% of the observed decline in mortality rates, while the remaining 52% can be attributed to the earlier diagnosis and treatment of other medical conditions.[77]

6.2 Alternative Mechanisms

Our analysis highlights changes in subcenter quality and person-hours as well as private provider quality as the main channels through which the CHOs improved health outcomes. Another potential mechanism is that the CHOs increased the presence of male primary healthcare workers at subcenters. While all of the existing ANMs are female, 64% of the new CHOs are male. This could especially benefit male patients who might feel uncomfortable visiting the ANM. Male providers might also be seen as more competent which could encourage an increase in overall take-up of healthcare services. We explore the importance of this channel in Appendix Table A26 by studying heterogeneity in treatment effects by CHO gender on average subcenter outcomes. While the coefficients tend to be larger for male CHOs, we cannot reject equality for six of the eight outcomes.[78] Unfortunately, the PCTS data does not allow us to analyze gender-specific mortality rates and our household survey data are too noisy to split by CHO and patient gender. We do, however, have gender-disaggregated data on the number of patients for treatment group subcenters from the Health and Wellness Center Portal. When we use this data, we find that, while male CHOs tend to increase the share of male patients visiting the subcenters, the relative differences are small in magnitude. Overall, this suggests that an increased presence of male primary healthcare

[77] The calculated contribution of the increase in acute heart disease and epilepsy patients assumes that these patients would die otherwise and should thus be seen as an upper bound.

[78] The only exceptions for which the differences in treatment effects are marginally significant are the effects on acute heart disease patients and the inverse hyperbolic sine of the elderly mortality rate.

workers at subcenters is not the main explanation for our results.

It could also have been possible that the CHOs could have improved health outcomes by encouraging patients to enroll in government health insurance. However, we observe no significant increase in the likelihood that a household is covered by healthcare insurance in our household survey data. An increase in health insurance take-up should have also led to an increase in hospitalizations, whereas we observe the opposite.

Since most of the CHOs are recent medical school graduates, part of the effect might also come from high levels of initial motivation. This raises the concern that the effects might weaken in the future as worker motivation declines. We do not have information on the CHO's previous work experience, but we can instead show that there are no differential effects for younger or older CHOs, suggesting that differences in motivation related to age or experience might not be a main factor.

Another potential explanation for the effects on private providers is that the presence of CHOs has changed patient demand. By experiencing higher-quality healthcare services in the public sector, patients might start to value quality more, which then increases the pressure on private providers to invest in quality upgrades as well. While we cannot rule out that this channel exists, the heterogeneity results by the number of private providers at baseline suggest that a decline in market power is the primary driver of the private sector responses.

6.3 Cost-Effectiveness Analysis

In the final part of the paper, we assess the cost-effectiveness of adding CHOs to subcenters and use our reduced-form estimates to examine how the cost-effectiveness of the reform varies across different assumptions.

For government costs, we consider increased salary and drug expenses. CHOs are paid USD 480 per month (including performance-based incentives). Since medicine is provided for free at public facilities, we also account for higher public spending on medicines. We assume that the average medicine cost per patient visit is USD 0.24. When assessing government benefits, we account for future reductions in government spending due to decreased hospitalizations. We use estimates from Garg et al. (2022) who calculate that average public spending per hospitalization episode is equal to USD 91.17. For private benefits, we consider the decline in all-age mortality as well as decreased out-of-pocket spending for hospitalizations. We follow Hendren and Sprung-Keyser (2020) and use USD 100,000 as the value of a statistical life year. For hospitalizations, we again use estimates from Garg et al. (2022) who calculate that average private spending per hospitalization episode is equal to USD 185.10.

Without accounting for the change in hospitalizations, we estimate a marginal value of public funds of 6.84. In other words, adding CHOs to subcenters generates USD 6.84 in private benefits for every government dollar spent. Additional calculations show that the reform costs USD 14,624 per life-year saved.[79] If we further assume a persistent decline in hospitalizations, we would even predict that the reform would pay for itself as the savings from reduced public spending on hospitalizations would be larger than the government's total costs. We also report cost-effectiveness results based on alternative assumptions in Appendix Table A27.

However, as discussed in Section 5.6, these average estimates hide substantial heterogeneity in the marginal effect of adding CHOs to subcenters. The bottom plot in Figure 6 shows that, among subcenters for which we predict that adding a CHO would lead to a decline in mortality rates, estimates for the costs per life-year saved range from USD 4,499 to USD 341,379 (ignoring changes in hospitalizations and drug costs).

7 Conclusion

While low- and middle-income countries have made substantial progress in improving maternal and child health outcomes in recent decades, the gap in life expectancy at age 60 between rich and poor countries has increased over the same period.[80] Looking ahead, the discrepancy could even widen as health systems in India and other low- and middle-income countries struggle to adapt to an aging population. In this paper, we examine how adding a mid-level healthcare worker to rural public village clinics affects elderly mortality outcomes. We use novel administrative and survey data and exploit the staggered rollout of a large-

[79] By comparison, Bailey and Goodman-Bacon (2015) estimate that Community Health Centers in the US cost USD 68,580 per life-year saved (after deflating their estimates to 2022 dollars). Medicaid costs between USD 204,470 and 582,930 per life-year saved (Chay et al., 2012).

[80] Life expectancy at age 60 increased from 16.6 to 22.9 years between 1960 and 2019 in high-income countries, but only increased from 14.4 to 18.2 years in low- and middle-income countries.

scale public healthcare reform to show that adding new healthcare workers to rural village clinics led to a substantial reduction in all-age mortality rates, driven by a decline in elderly deaths. We find that the labor inputs achieved these effects by simultaneously improving public sector quality and access and also raising private provider quality. We further demonstrate that a reallocation of the new healthcare workers could have achieved an even greater mortality reduction.

Our findings imply that policies that use non-physician practitioners to strengthen public healthcare provision can be low-hanging fruits to improve health outcomes, especially for governments in low- and middle-income countries that face tight health budgets and have limited state capacity. Similar healthcare cadres are already used in 37 countries in Africa and Asia (Desai et al., 2020), making the results also relevant outside of India.

More broadly, our results provide direct evidence that local market power of private providers contributes to the low-quality equilibrium in healthcare markets and that improving public sector capacity through labor inputs can be one tool to improve this situation. Furthermore, the idea that quality improvements might not be sufficient if consumers do not value these changes is also relevant in many other settings. In such cases, governments might want to accompany quality initiatives with additional changes that are more highly valued or more easily observable by consumers to reach a larger population.

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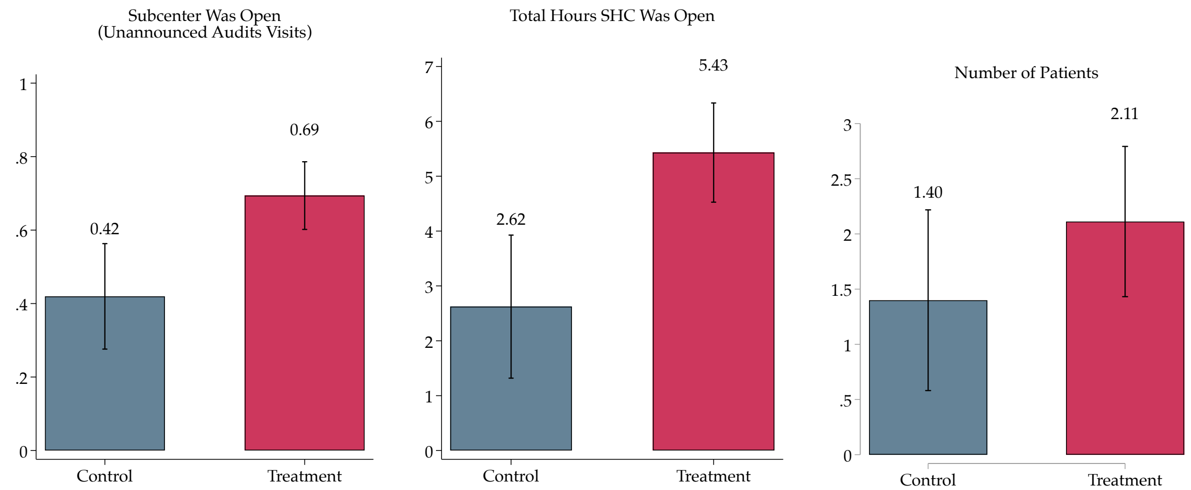
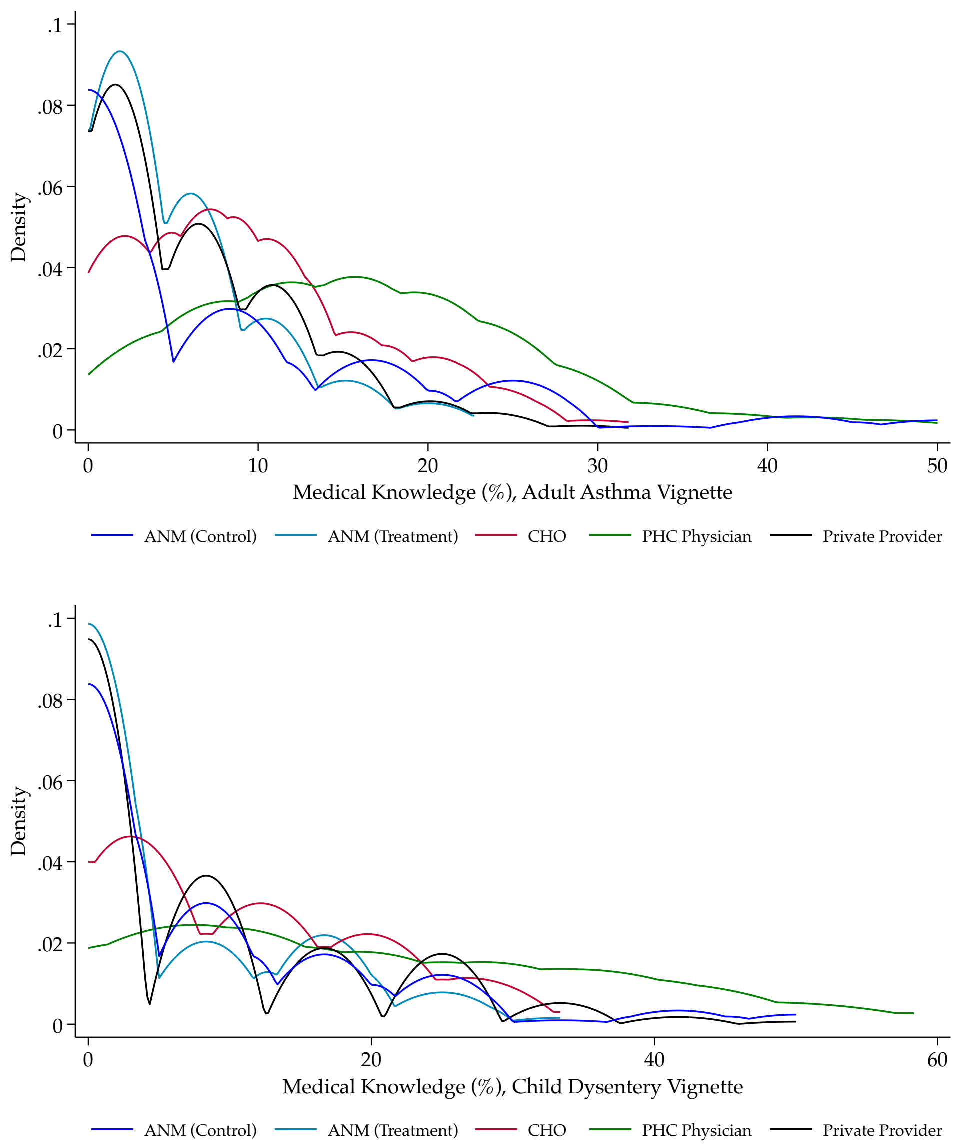
Figure 1: Results from Unannounced Audit Visits



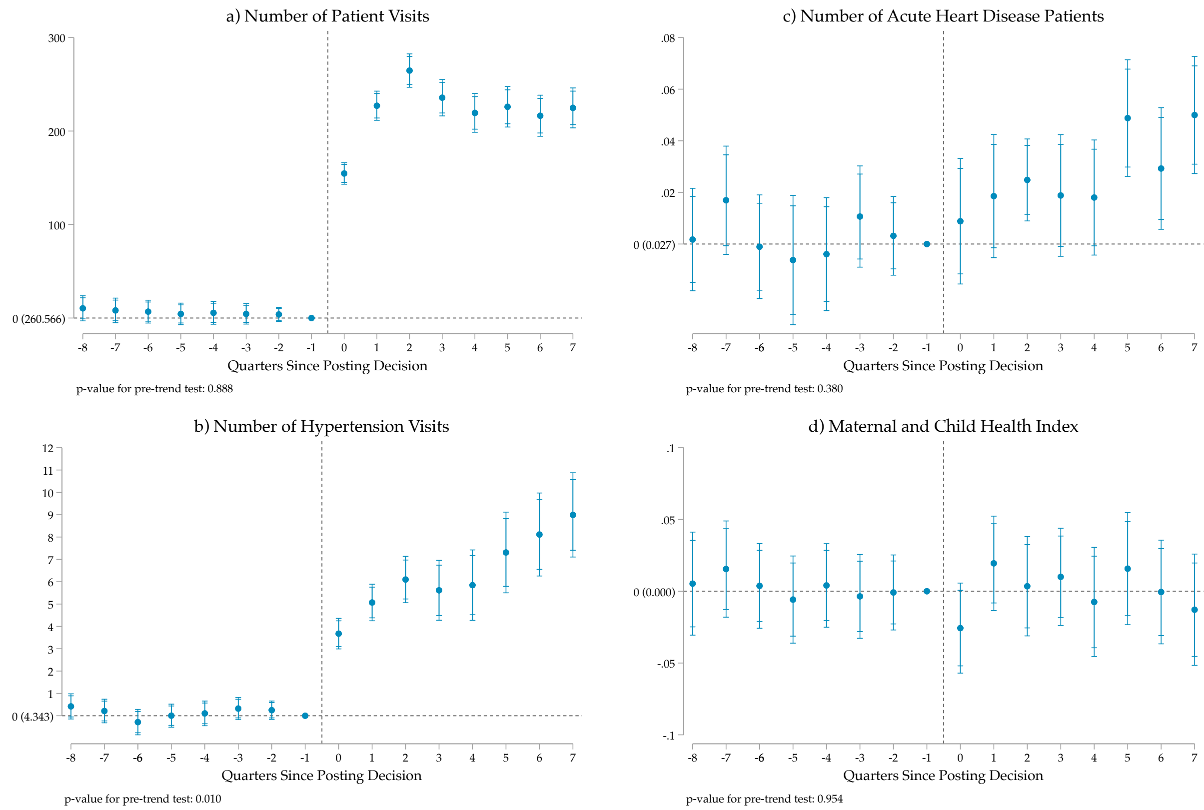
Notes: The figure presents the results from unannounced visits conducted between March and June 2023. The left figure shows the share of subcenters that were open at least at some point during the day of the unannounced visit. The middle figure shows the average number of hours for which the subcenters were open. The right figure shows the number of patients observed to have visited the subcenter. The whiskers correspond to 95% confidence intervals. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. The sample consists of 94 control group subcenters and 98 treatment group subcenters.

Figure 2: Checklist Completion Rates Across Providers by Vignette



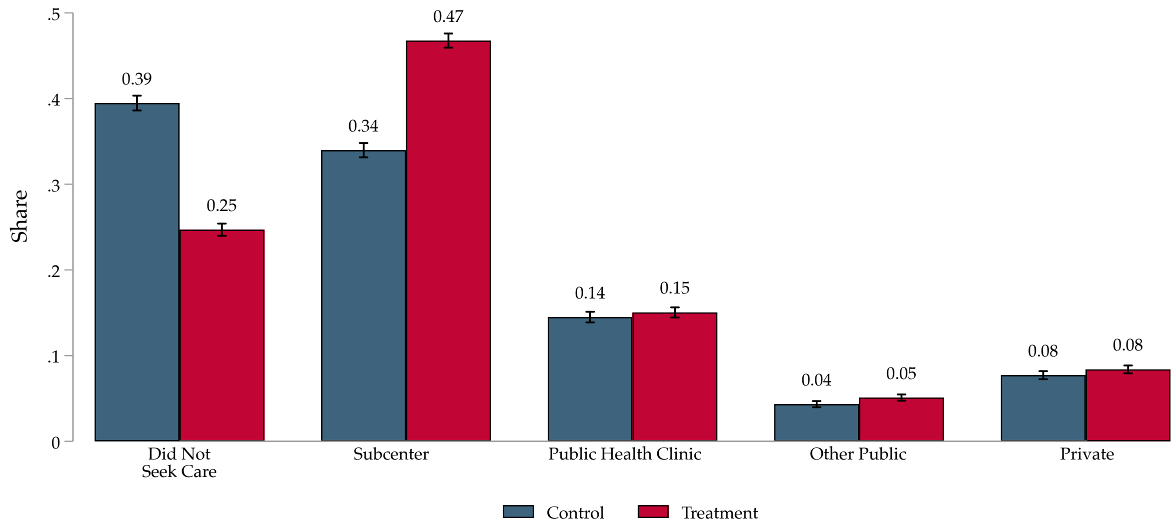
Notes: The figure shows the distribution of medical knowledge for different healthcare providers during the endline survey, separately for the adult asthma and the child dysentery vignette. Medical knowledge is measured as the average checklist completion rates. The sample consists of 97 control group ANMs, 96 treatment group ANMs, 96 CHOs, 49 public clinic physicians, and 207 private providers.

Figure 3: Effects of Community Health Officers on Healthcare Services



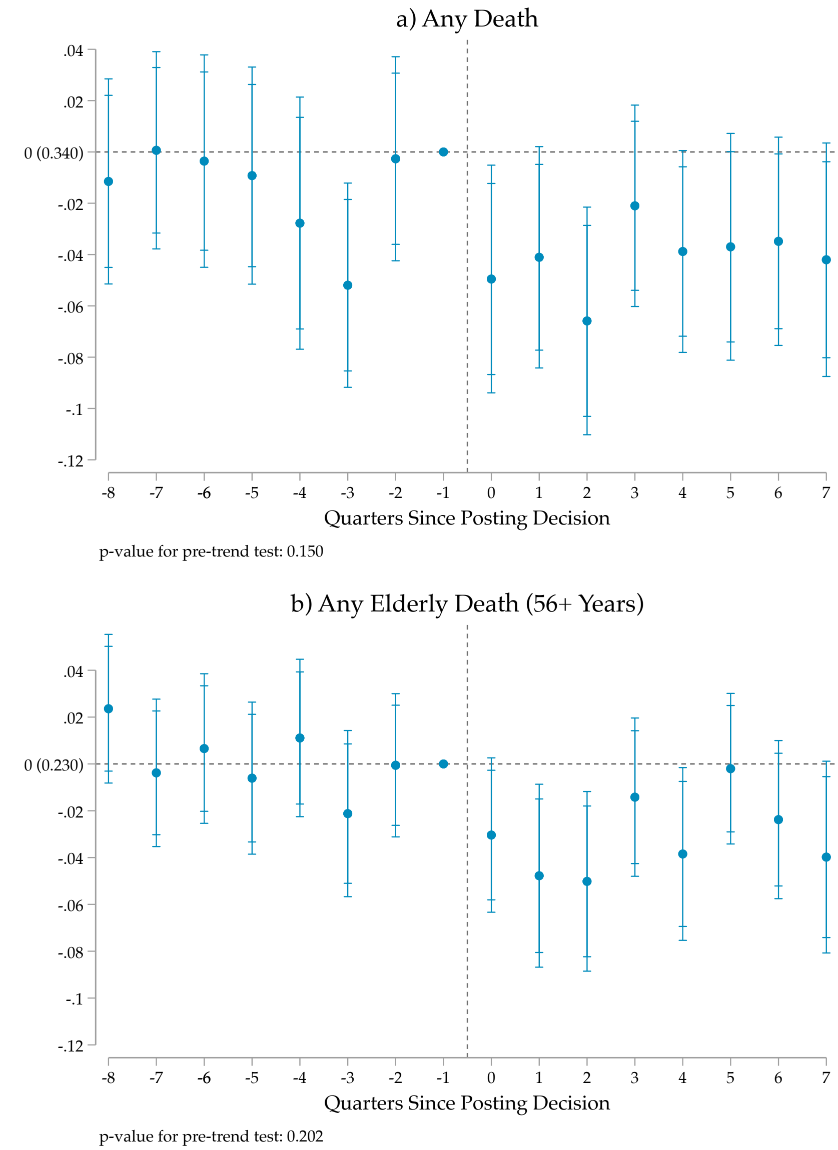
Notes: The figure shows weighted regression estimates of the effect of CHOs on healthcare services. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and quarter dummies, and year and subcenter fixed effects. The base quarter (Q1 2022) is omitted. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The figures show 90 and 95% confidence intervals based on standard errors clustered at the subcenter level. The number in parentheses on the y-axis shows the treatment group mean of the outcome in the base quarter. The p-value at the bottom of each figure corresponds to a chi-squared test of the null hypothesis that all interactions between treatment and pre-period dummies are statistically equal to zero. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System.

Figure 4: Healthcare Utilization and Provider Choices by Treatment



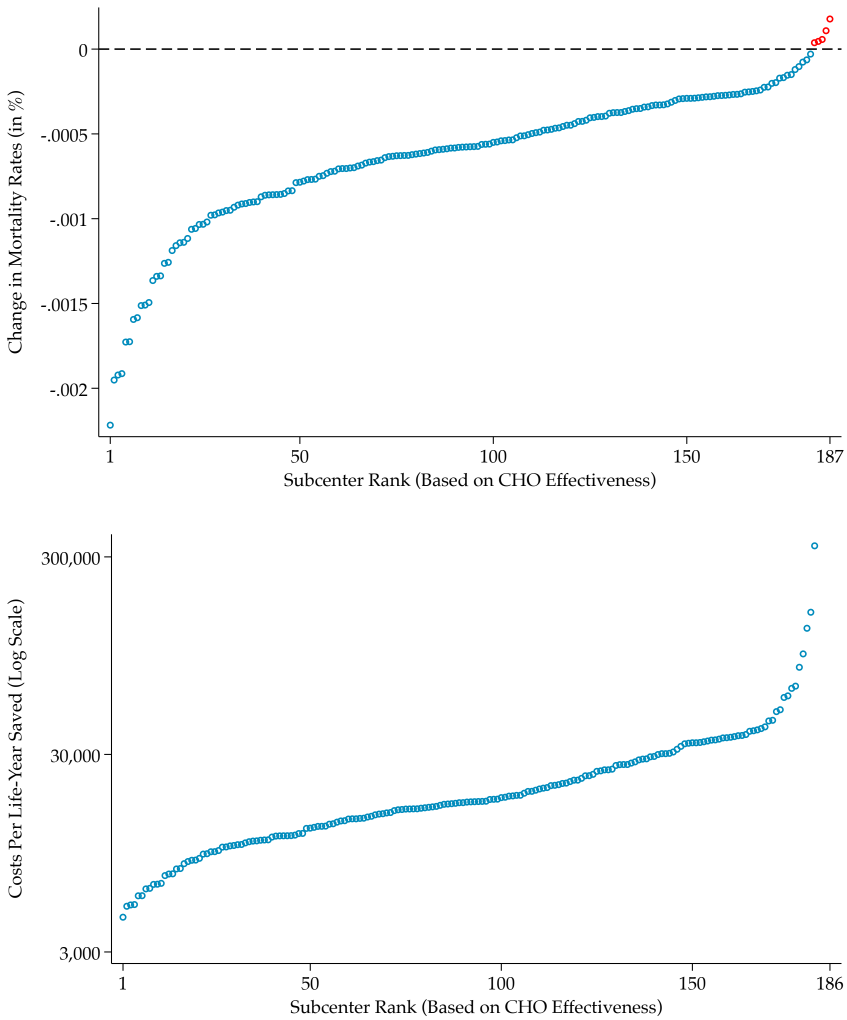
Notes: The figure shows healthcare provider choices in treatment and control group areas. The whiskers correspond to 95% confidence intervals. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 26,496 respondents (across 1,603 subcenters) who reported having at least one symptom in the past 30 days and were surveyed between August 2023 and June 2024. Outcomes are obtained from the CHIP household census data.

Figure 5: Effects of Community Health Officers on Health Outcomes



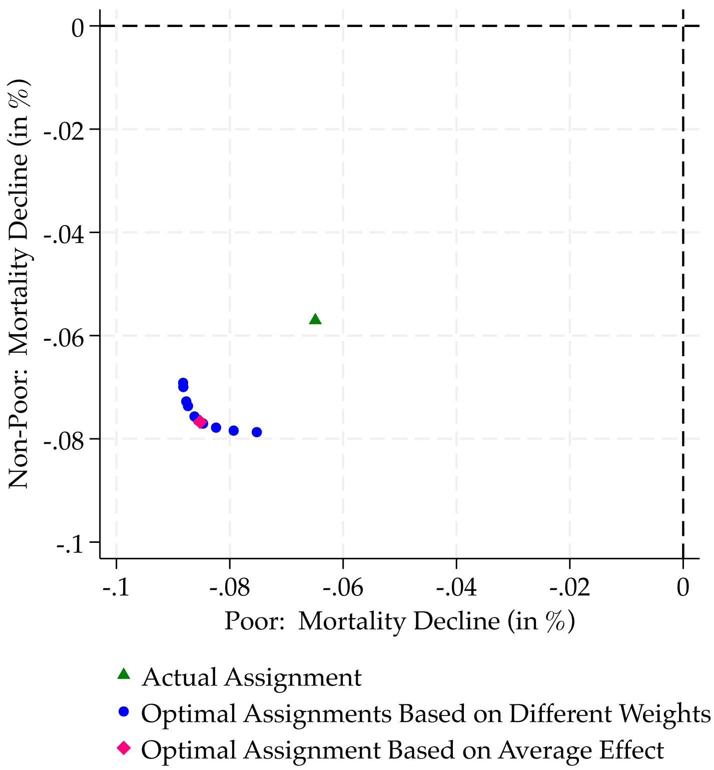
Notes: The figure shows weighted regression estimates of the effect of CHOs on health outcomes. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and quarter dummies, and year and subcenter fixed effects. The base quarter (Q1 2022) is omitted. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The figures show 90 and 95% confidence intervals based on standard errors clustered at the subcenter level. The number in parentheses on the y-axis shows the treatment group mean of the outcome in the base quarter. The p-value at the bottom of each figure corresponds to a chi-squared test of the null hypothesis that all interactions between treatment and pre-period dummies are statistically equal to zero. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. Outcomes for other age groups are shown in Appendix Figure A10.

Figure 6: Differences in Marginal Effects of CHOs Based on Counterfactual Simulations



Notes: The first figure shows the marginal effect of posting a CHO on predicted changes in mortality rates across all sample markets. Estimates are based on posting a CHO to a particular subcenter relative to the baseline scenario where no CHOs are posted at all. Red dots indicate subcenters for which the posting of a CHO leads to an increase in predicted mortality rates. The second plot is restricted to subcenters for which mortality rates decline. The cost calculations in this exercise only account for CHO salaries.

Figure 7: Trade-off Between Targeting Average Impacts vs. Health Equity



Notes: The figure shows predicted changes in mortality rates for poor and non-poor households across different counterfactual scenarios. The green triangle corresponds to the actual assignment of CHOs by the government. The blue dots represent eleven optimal assignment rules based on how much weight is given to poor patients (ranging from 0% to 100% in 10% intervals). The pink diamond represents the optimal assignment rule when targeting the average effect.

Table 1: Comparison of Treatment and Control Areas

	Original Sample				Reweighted Sample				
	Control Mean (1)	Control St. D. (2)	Treatment Coeff. (3)	Treatment St. E. (4)	Control Mean (5)	Control St. D. (6)	Treatment Coeff. (7)	Treatment St. E. (8)	N (9)
Panel A: Targeted Characteristics									
Distance to District HQ (in km)	71.90	[37.68]	-3.51***	(1.06)	68.13	[36.72]	0.25	(1.52)	4,909
Distance to Subdistrict HQ (in km)	25.63	[17.59]	-2.73***	(0.45)	23.07	[13.93]	-0.17	(0.55)	4,909
Panel B: Catchment Area Characteristics									
Distance to Public Health Clinic (in km)	8.18	[6.67]	-1.22***	(0.18)	7.23	[5.92]	-0.27	(0.24)	4,909
Total Population	2965.46	[1514.49]	150.15***	(44.47)	3028.62	[1595.32]	86.99	(70.90)	4,909
Elderly Population Share	0.09	[0.04]	0.00	(0.00)	0.09	[0.04]	0.00	(0.00)	4,858
Scheduled Caste Share	0.19	[0.15]	-0.02***	(0.00)	0.16	[0.12]	0.01**	(0.00)	4,909
Scheduled Tribe Share	0.17	[0.28]	0.02***	(0.01)	0.20	[0.31]	-0.00	(0.01)	4,909
Female Share	0.49	[0.02]	0.00*	(0.00)	0.49	[0.02]	-0.00	(0.00)	4,909
Literacy Rate	0.47	[0.10]	0.03***	(0.00)	0.50	[0.11]	0.01	(0.00)	4,909
Land Ownership Rate	0.69	[0.19]	0.04***	(0.01)	0.72	[0.18]	0.00	(0.01)	4,859
Employment Rate	0.50	[0.08]	-0.01***	(0.00)	0.49	[0.09]	0.00	(0.00)	4,909
(Imputed) Consumption per Capita (in INR)	16525.32	[3706.49]	651.17***	(109.91)	16954.37	[3985.32]	222.12	(163.57)	4,859
Panel C: Average Facility Indicators in Q1 2022									
Number of Patients	260.57	[216.24]	-22.55***	(5.61)	241.70	[205.98]	-3.69	(8.60)	4,909
Number of Acute Heart Disease Patients	0.03	[0.21]	-0.00	(0.01)	0.03	[0.21]	-0.00	(0.01)	4,909
Number of Hypertension Patients	4.34	[10.49]	-0.39	(0.27)	4.34	[9.56]	-0.39	(0.33)	4,909
Maternal and Child Health Services Index	-0.00	[0.82]	0.03	(0.02)	-0.01	[0.81]	0.04	(0.04)	4,909
All-Age Mortality Rate	0.38	[0.73]	-0.01	(0.02)	0.38	[0.74]	-0.01	(0.03)	4,909
Elderly Mortality Rate	2.39	[5.54]	0.06	(0.16)	2.60	[6.12]	-0.16	(0.27)	4,909

Notes: This table shows the means of selected covariates for the original and reweighted sample. Panel A reports on the covariates used to estimate the propensity score for the inverse probability weighting. Panel B reports on covariates at the catchment area level. Catchment-level covariates are calculated based on population-weighted averages across all villages in the catchment area of the respective subcenter. Panel C reports on the main outcomes in the pre-treatment reference period. Columns (1)-(4) present the original sample and columns (5)-(8) present the reweighted sample. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. Differences in sample sizes across variables reflect missing data. Columns (1) and (5) report the control mean of the dependent variable for each relevant sample. Columns (3), and (7) report the difference in the dependent variable from OLS regressions of each outcome on an indicator variable for CHO assignment. The sample consists of 4,909 subcenters in our administrative data.

Table 2: Effects of Community Health Officers on Subcenter Characteristics

	Survey Sample						Admin Data Sample
	Number of Providers	Quality Index	Number of Community Health Workers	Electricity	Equipment Index	Medicine Index	CHO Posted
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment × Post	0.995***	0.680***	-0.070	0.022	-0.172	-0.223	0.846***
	(0.025)	(0.096)	(0.122)	(0.057)	(0.417)	(0.346)	(0.011)
Control Group Mean (Baseline)	1.000	0.013	2.615	0.588	7.417	3.848	0.000
Treatment Group Mean (Baseline)	1.011	-0.039	2.947	0.628	7.484	3.661	0.000
Counterfactual Treatment Group Mean (Endline)	1.035	0.010	2.998	0.665	6.860	4.078	0.154
Observations	378	378	376	374	282	282	9,818

Notes: This table shows the effects of CHOs on subcenter staffing and infrastructure. We regress the outcome on the treatment dummy, the post-period dummy, and an interaction between both of them. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. In Columns (1)-(6), the sample consists of 193 subcenters and outcomes are obtained from ANM and CHO surveys. In Column (7), the sample consists of 4,909 subcenters and information on CHO postings are obtained from the Health and Wellness Center Portal. The reason that treatment assignment does not perfectly predict CHO presence is that 15% of control group subcenters also received a CHO in December 2022. Appendix Table A4 provides regression estimates for each index component in the quality index in Column (2).

Table 3: Effects of Community Health Officers on Patient Visits

	Total Number of Patient Visits	Type of Visits: Acute Heart Disease	Stroke	Epilepsy	Hypertension	Diabetes	Maternal & Child Health Services Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment × Post	215.622***	0.024***	-0.002	0.012**	6.211***	4.739***	-0.002
	(7.584)	(0.007)	(0.005)	(0.006)	(0.565)	(0.470)	(0.011)
Control Group Mean (Q1 2022)	237.503	0.036	0.022	0.034	3.731	3.114	-0.012
Treatment Group Mean (Q1 2022)	239.272	0.038	0.026	0.033	3.470	2.947	0.033
Counterfactual Treatment Group Mean (Post-Periods)	370.741	0.036	0.046	0.031	8.646	7.659	0.146
Observations	9,818	9,818	9,818	9,818	9,818	9,818	9,818

Notes: This table shows the aggregate effects of CHOs on healthcare services. The regression coefficients are estimated by pooling the pre- and post-periods and regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Appendix Table A7 provides regression estimates for each index component in the maternal and child health index in Column (7). See Data Appendix for details on variable definitions.

Table 4: Effects of Community Health Officers on Mortality Outcomes

	Any Death (1)	Number of Deaths (2)	Mortality Rate (3)	Mortality Rate (IHS) (4)
Panel A: All-Age Deaths				
Treatment × Post	-0.028**	-0.102**	-0.039**	-0.031**
	(0.011)	(0.041)	(0.019)	(0.013)
Control Group Mean (Pre-Periods)	0.362	1.071	0.409	0.311
Treatment Group Mean (Pre-Periods)	0.372	1.123	0.409	0.314
Counterfactual Treatment Group Mean (Post-Periods)	0.361	1.057	0.392	0.307
Panel B: Elderly Deaths (56+ Years)				
Treatment × Post	-0.032***	-0.081***	-0.339**	-0.095***
	(0.010)	(0.031)	(0.150)	(0.030)
Control Group Mean (Pre-Periods)	0.228	0.596	2.650	0.660
Treatment Group Mean (Pre-Periods)	0.246	0.627	2.641	0.695
Counterfactual Treatment Group Mean (Post-Periods)	0.257	0.635	2.707	0.727
Panel C: Other Age Groups				
Treatment × Post	-0.008	-0.018	-0.009	-0.007
	(0.010)	(0.020)	(0.008)	(0.007)
Control Group Mean (Pre-Periods)	0.262	0.466	0.180	0.158
Treatment Group Mean (Pre-Periods)	0.267	0.482	0.180	0.159
Counterfactual Treatment Group Mean (Post-Periods)	0.229	0.406	0.156	0.137
Observations	9,818	9,818	9,818	9,818

Notes: This table shows the aggregate effects of CHOs on mortality outcomes. The regression coefficients are estimated by pooling the pre- and post-periods and regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table 5: Effects of Community Health Officers on Health Outcomes in Household Surveys

	All Household Members			
	Past 30 Days		Past 6 Months	
	Any Symptoms (1)	Medical Expenses (2)	Any Hospitalization (3)	Hospital Days (4)
Treatment × Post	0.013	-1.572	-0.017**	-0.052**
	(0.031)	(48.503)	(0.008)	(0.021)
Control Group Mean (Baseline)	0.098	118.515	0.024	0.057
Treatment Group Mean (Baseline)	0.092	148.571	0.036	0.090
Counterfactual Treatment Group Mean (Endline)	0.171	234.342	0.033	0.089
Observations	5,846	5,792	5,847	5,846

Notes: This table shows the effects of CHOs on household survey outcomes at the household member level. The regression coefficients are estimated by regressing the outcome on the treatment dummy, the post-period dummy, and an interaction between both of them. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. See Data Appendix for details on variable definitions.

Table 6: Heterogeneity by Change in Quality and Person-Hours

	Number of Patient Visits (1)	Any Death (2)	All-Age Mortality Rate (3)	All-Age Mortality Rate (IHS) (4)	Number of Patient Visits (5)	Any Death (6)	All-Age Mortality Rate (7)	All-Age Mortality Rate (IHS) (8)
Treatment × Post × Δ Quality Index ≥ Median	266.577***	-0.097*	-0.172***	-0.133***				
	(30.242)	(0.054)	(0.056)	(0.046)				
Treatment × Post × Δ Quality Index < Median	254.510***	-0.068	-0.077	-0.064				
	(27.360)	(0.051)	(0.047)	(0.039)				
Treatment × Post × Δ Person-Hours ≥ Median					296.041***	-0.066	-0.046	-0.042
					(29.400)	(0.055)	(0.067)	(0.051)
Treatment × Post × Δ Person-Hours < Median					233.038***	-0.122**	-0.080	-0.070
					(26.869)	(0.049)	(0.063)	(0.048)
p-value: Coef 1 = Coef 2	0.733	0.597	0.074	0.103	0.067	0.300	0.526	0.518
Treatment (≥ Median) :								
Treatment Group Mean (Pre-Periods)	256.418	0.424	0.385	0.313	238.289	0.339	0.273	0.229
Counterfactual Treatment Group Mean (Post-Periods)	299.760	0.385	0.406	0.326	281.631	0.300	0.214	0.186
Observations	92	92	92	92	90	90	90	90
Treatment (< Median) :								
Treatment Group Mean (Pre-Periods)	236.073	0.288	0.198	0.171	249.231	0.394	0.315	0.265
Counterfactual Treatment Group Mean (Post-Periods)	279.415	0.249	0.220	0.185	292.573	0.355	0.256	0.221
Observations	86	86	86	86	104	104	104	104
Control Group:								
Control Group Mean (Pre-Periods)	240.975	0.343	0.246	0.207	240.003	0.349	0.288	0.240
Observations	182	182	182	182	182	182	182	182

Notes: This table shows the effects of CHOs on healthcare services and health outcomes by changes in subcenter quality and person-hours. In Columns (1)-(5), the regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and a dummy for whether the difference in the quality index between baseline and endline in treated areas is equal to or larger than the median difference, and year and subcenter fixed effects. In Columns (5)-(8), the regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and a dummy for whether the difference in subcenter person-hours between baseline and endline in treatment areas is equal to or larger than the median difference, and year and subcenter fixed effects. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System, and changes in the quality index and person-hours are based on surveys with ANMs and CHOs. See Data Appendix for details on variable definitions.

Table 7: Effects of Community Health Officers on Private Provider Behavior

	Number of Providers (1)	Number of Patients (2)	Typical Fee (3)	Opening Hours (4)	Quality Index (5)
Panel A: Pooled					
Treatment × Post	0.155	-1.724	6.028	-4.836	0.291**
	(0.099)	(23.940)	(16.891)	(5.206)	(0.136)
Control Group Mean (Baseline)	1.446	94.810	97.806	52.992	0.204
Treatment Group Mean (Baseline)	1.092	112.859	103.291	54.870	-0.023
Counterfactual Treatment Group Mean (Endline)	0.967	127.672	103.411	51.933	-0.228
Observations	384	340	346	334	363
Panel B: Heterogeneity by Number of Providers at Baseline					
Treatment × Post × 1 Private Provider at Baseline	0.064	-30.848	-12.964	9.746	0.642**
	(0.125)	(33.470)	(24.149)	(9.870)	(0.260)
Treatment × Post × $\geq$ 2 Private Providers at Baseline	0.101	10.320	8.257	-7.794	0.084
	(0.216)	(32.464)	(20.839)	(6.227)	(0.162)
p-value: Coef 1 = Coef 2	0.881	0.379	0.507	0.136	0.071
Only One Private Provider at Baseline :					
Control Group Mean (Baseline)	1.073	71.037	64.627	61.080	0.077
Treatment Group Mean (Baseline)	0.949	123.300	91.034	55.103	-0.077
Counterfactual Treatment Group Mean (Endline)	1.013	150.897	121.867	35.096	-0.476
Observations	130	118	118	114	123
At Least Two Private Provider at Baseline:					
Control Group Mean (Baseline)	2.112	103.336	109.418	50.607	0.245
Treatment Group Mean (Baseline)	2.107	106.333	110.400	54.716	0.008
Counterfactual Treatment Group Mean (Endline)	1.899	120.152	101.558	56.627	-0.075
Observations	114	215	221	213	233

Notes: This table shows the effects of CHOs on private provider outcomes. In Panel A, we regress the outcome on the treatment dummy, survey round dummies, and an interaction between the treatment dummy and the post-period dummy. In Panel B, we regress the outcome on the treatment dummy, the post-survey dummies, and an interaction between the treatment dummy and the post-period dummy, separately for catchment areas with only one or at least two private providers at baseline. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. Outcomes are obtained from our private provider surveys. Appendix Table A18 provides regression estimates for each index component in the quality index in Column (5). See Data Appendix for details on variable definitions.

Table 8: Demand Estimates

Parameters	Estimate	SE
Quality Index (in Standard Deviations)	0.425	(0.687)
Quality $\times$ Non-Poor	0.112	(0.765)
Person-Hours (in 10 Person-Hours)	0.277	(0.100)
Price (in INR 100)	-0.921	(0.517)
Price $\times$ Non-Poor	0.770	(0.861)
Distance (in km)	-0.463	(0.284)
Distance $\times$ Non-Poor	0.051	(0.428)
Equipment Index	0.084	(0.267)
Medicine Index	0.463	(0.376)
Years of Experience	0.001	(0.009)
Years Working in the Village	0.003	(0.011)
Private	-0.548	(0.454)
PHC	-0.305	(0.479)
σ	0.788	(0.634)
Constant	-3.933	(0.818)

Notes: This table reports the results from the estimation of the demand model.

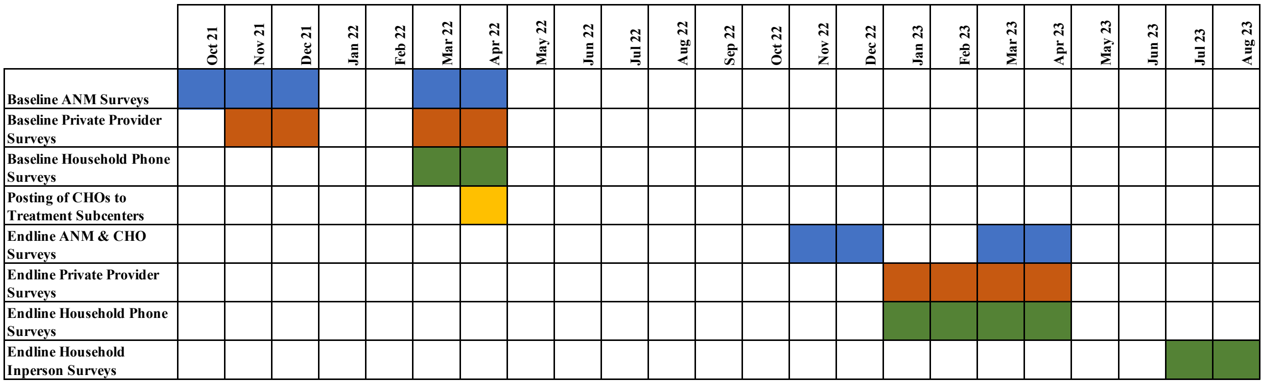
Table 9: Counterfactual Analysis

Counterfactuals	Market Shares			Average Quality	Δ All-Age Mortality Rate (in %)
	Subcenter	PHC	Private		
	(1)	(2)	(3)	(4)	(5)
Baseline					
1) Baseline	0.100	0.327	0.100	1.253	
Decomposition					
2) Full Treatment Effect ($\uparrow q_{shc}$,$\uparrow h_{shc}$,$\uparrow q_{priv}$)	0.265	0.275	0.092	1.544	-0.103
3) Only Increase in Subcenter Quality ($\uparrow q_{shc}$)	0.129	0.318	0.096	1.351	-0.034
4) Only Increase in Subcenter Person-Hours ($\uparrow h_{shc}$)	0.217	0.293	0.086	1.321	-0.024
5) No Effects on Private Providers ($\uparrow q_{shc}$,$\uparrow h_{shc}$)	0.268	0.278	0.081	1.516	-0.093
Private Sector Ban					
6) Ban Without CHOs	0.111	0.351	0.000	1.214	0.014
7) Ban With CHOs	0.292	0.293	0.000	1.509	-0.090
Reallocation of CHOs (51% of SHCs Get Treated)					
8) Actual	0.198	0.296	0.094	1.424	-0.060
9) Random	0.190	0.298	0.095	1.413	-0.056
10) Random (Within-Markets)	0.192	0.298	0.095	1.413	-0.058
11) Rule: Poorest Catchment Areas	0.187	0.304	0.095	1.415	-0.057
12) Rule: Areas with 1 Private Provider	0.191	0.298	0.099	1.431	-0.063
13) Rule: Largest Catchment Area Population	0.201	0.298	0.099	1.431	-0.064
14) Rule: Furthest from PHC	0.181	0.310	0.095	1.424	-0.060
15) Rule: Subcenter Quality at Baseline	0.206	0.293	0.094	1.449	-0.069
16) Optimal	0.210	0.298	0.095	1.480	-0.080
17) Optimal (Within-Markets)	0.207	0.294	0.096	1.454	-0.071

Notes: This table presents the results of the counterfactual analysis. The different scenarios are as follows. Row (1): the baseline model. Row (2): full treatment effect in which subcenter quality and person-hours increase and private providers improve their quality in all subcenter locations. Row (3): only increase in subcenter quality, no change in subcenter person-hours or private provider quality. Row (4): only increase in subcenter person-hours, no change in subcenter or private provider quality. Row (5): increase in subcenter quality and person-hours, but no change in private provider quality. Row (6): ban on private providers, no change in subcenter quality and person-hours. Row (7): ban on private providers and increase in subcenter quality and person-hours in all subcenter locations. Row (8): 96 out of the 187 sample SHCs receive a CHO as per the observed government assignment. Row (9)-(10): average outcomes across 100 random allocations of the 96 CHOs within the same markets or across markets. Row (11)-(15): rule-based assignments based on the stated priority criteria. Row (12) first chooses areas with one private provider and then randomly chooses another clinics until 96 CHOs are assigned. Row (16)-(17): Optimal allocation based on the objective to maximize the decline in all-age mortality rates. Row (15) only reallocates CHOs within markets. Columns (1)-(3) show the average market shares for subcenters, PHCs, and private providers. The market share of the outside option is omitted. Column (4) reports the average healthcare quality of the chosen provider, with quality defined as 0 if the outside option is chosen. Column (5) reports the predicted relative decline in all-age mortality rates based on the changes in average quality. The data comes from our survey sample.

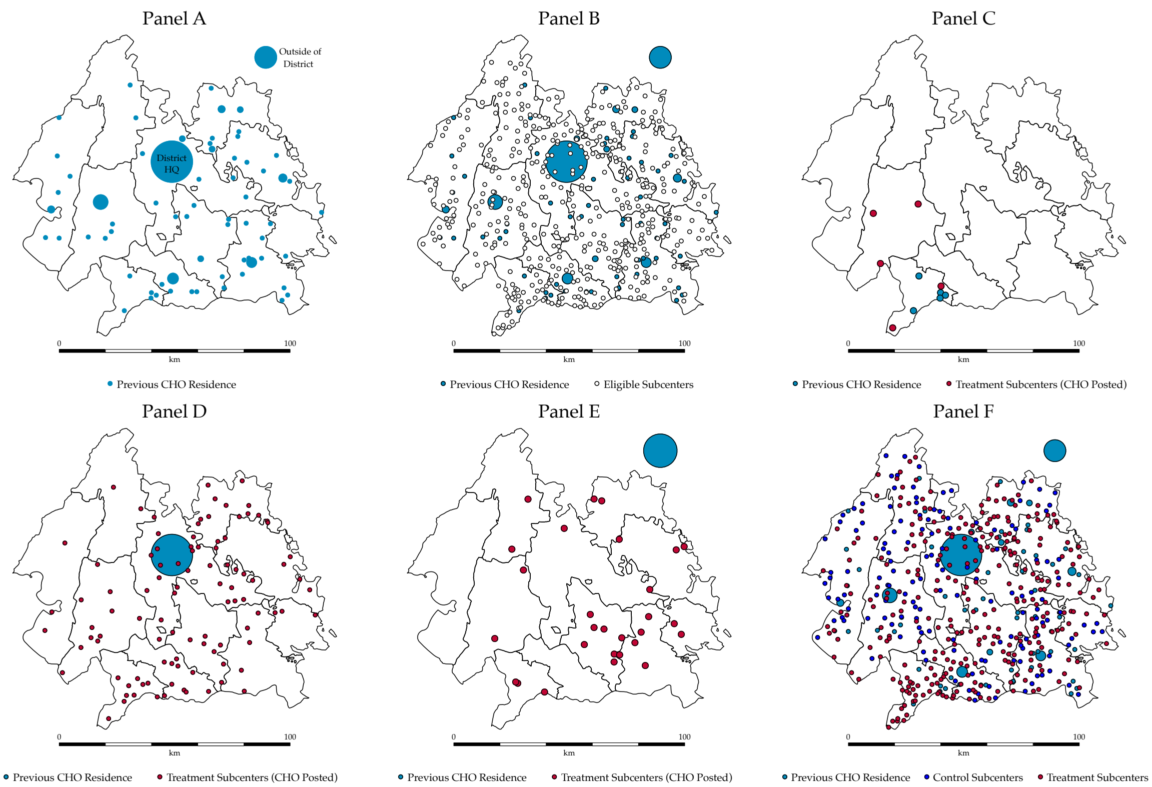
A. Appendix Tables and Figures

Figure A1: Timeline



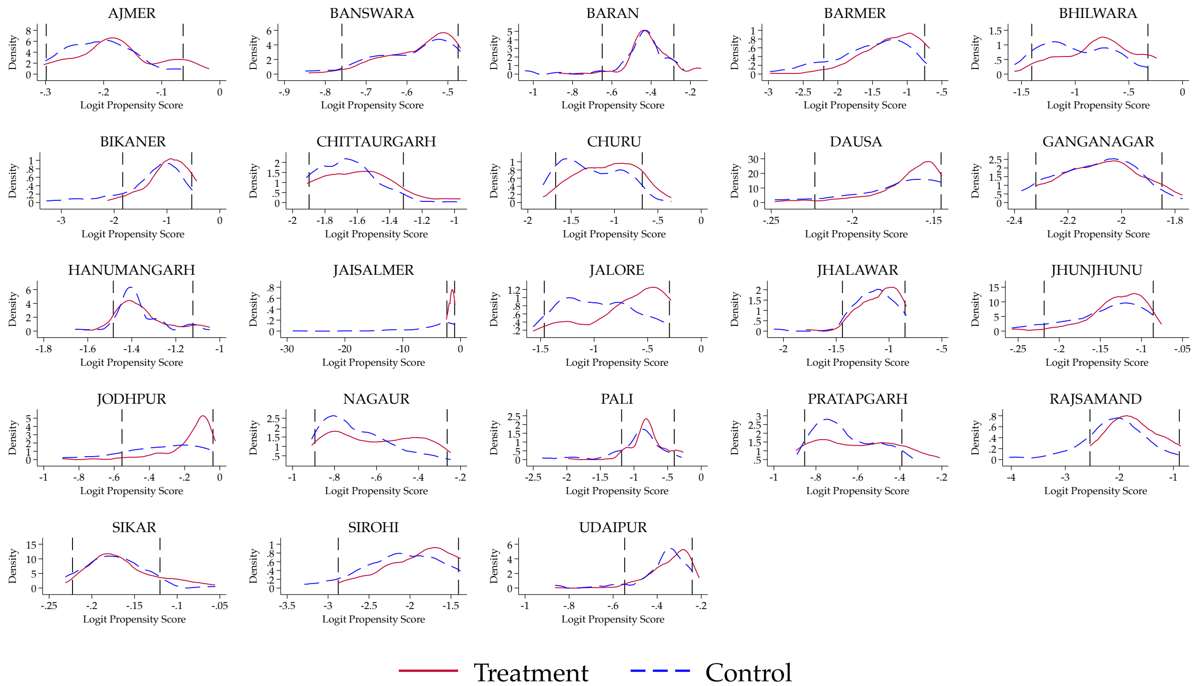
Notes: This figure shows the timeline for the primary data collection. The assignments of CHOs to subcenters were announced on March 27, 2022. Most CHOs started to work in the field by the end of April 2022. Household in-person surveys at endline were done with households that were surveyed over the phone at baseline but could not be reached over the phone at endline. See Appendix Table A2 for survey completion rates.

Figure A2: Assignments of CHOs Across Udaipur District



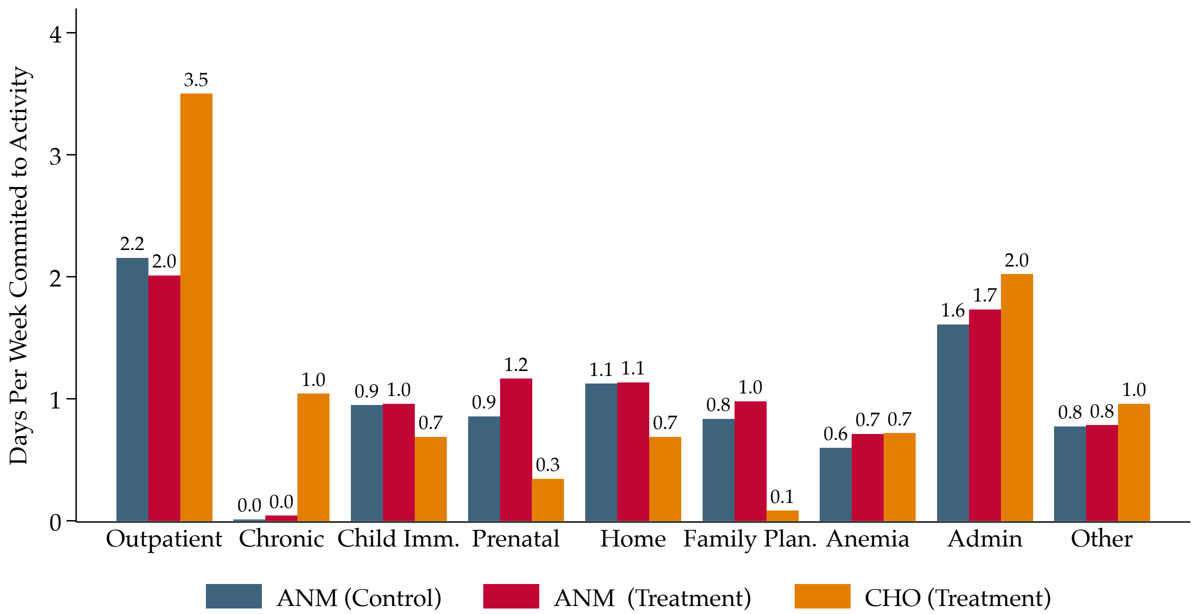
Notes: The figures show where CHOs in Udaipur district previously resided as well as the location of treatment and control group subcenters in the district. The boundaries represent subdistricts. The top-left figure shows where CHOs resided before their postings. The size of each bubble represents the number of CHOs in each location. The top-middle figure shows the number of subcenters that were eligible to receive a CHO. The top-right figure indicates the assignment locations for the CHOs that previously resided in Kherwara subdistrict. The bottom-left figure indicates the assignment locations for the CHOs that previously resided in Udaipur city, the district headquarter. The bottom-middle figure indicates the assignment locations for the CHOs that previously resided outside of Udaipur district. The bottom-right figure shows the location of all treatment and control group subcenters in the district. Information on previous residence locations are obtained from surveys with 243 CHOs.

Figure A3: Common Support Restrictions



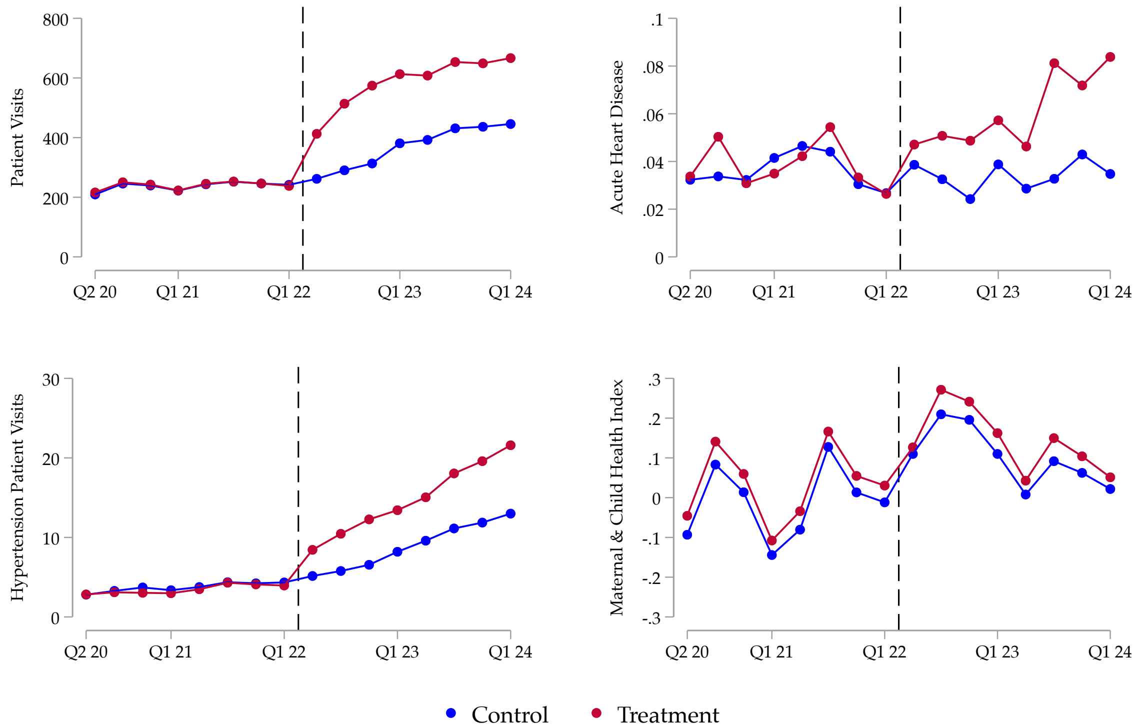
Notes: This figure shows the distribution of the logit propensity score in each district within our sample for treatment and control group facilities. Districts in which more than 90% of converted subcenters received a CHO are omitted. Propensity scores are estimated by a logistic regression that regresses the treatment dummy on linear and squared terms of the subcenter's distance to the district and block headquarters. In our preferred specification, we implement common support restrictions by excluding observations within the top or bottom 2.5 percent of either the control or treatment propensity score distribution in each district (vertical lines). See Appendix Table A12 for robustness regarding alternative common support restrictions.

Figure A4: Time Use of ANMs and CHOs



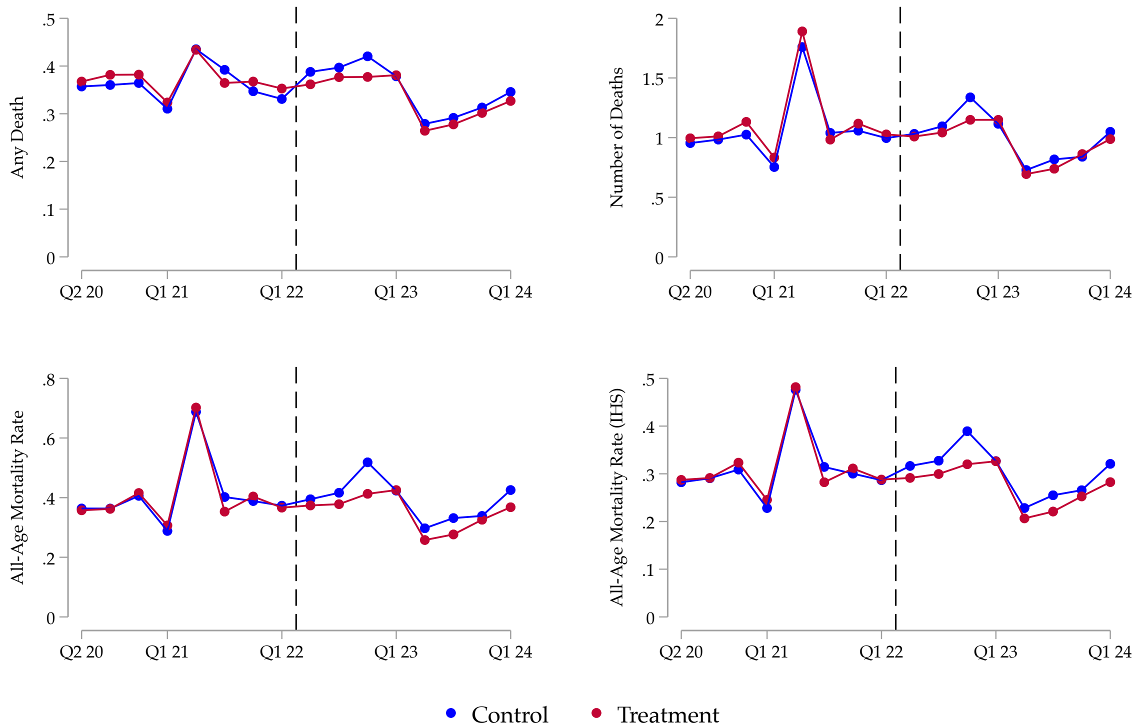
Notes: The figure shows the weighted average number of days per week treatment and control group ANMs and CHOs spent on different activities according to the time-use module in the endline survey. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. Respondents could select more than one activity per day, so the aggregate number of days can sum up to more than 7 days. The sample consists of 97 control group ANMs, 96 treatment group ANMs, and 96 CHOs.

Figure A5: Trends in Patient Visits by Treatment Group



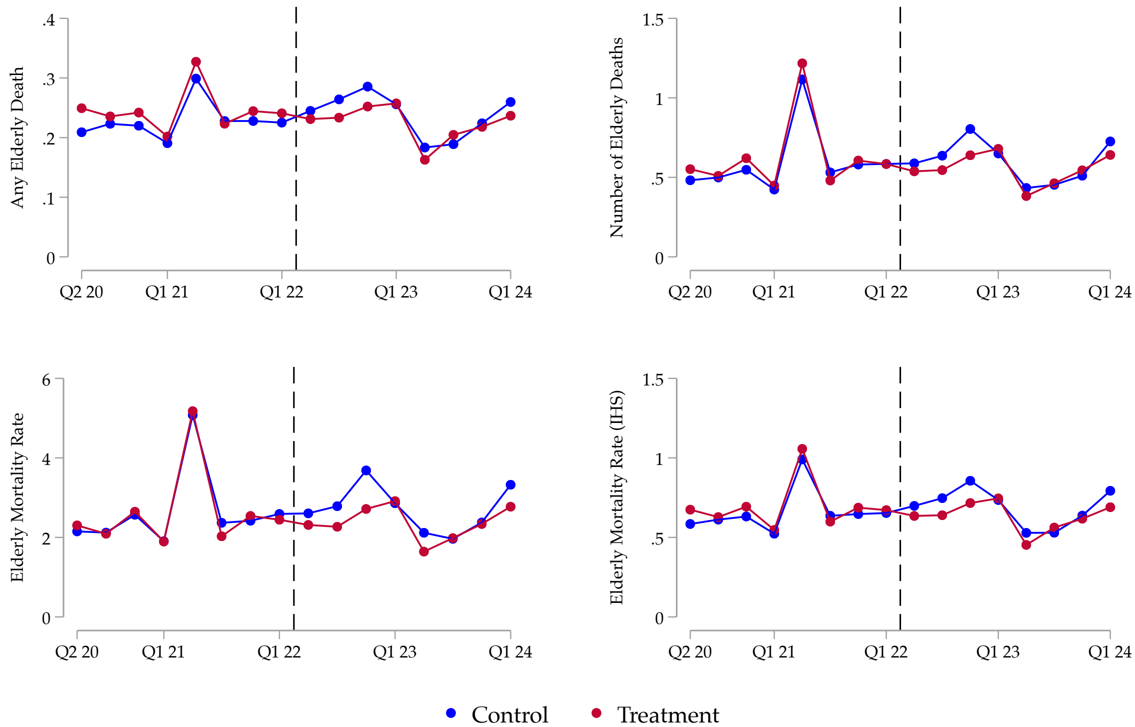
Notes: The figure shows weighted means for our patient visit outcomes for treatment and control group subcenters over time. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System.

Figure A6: Trends in All-Age Mortality Outcomes by Treatment Group



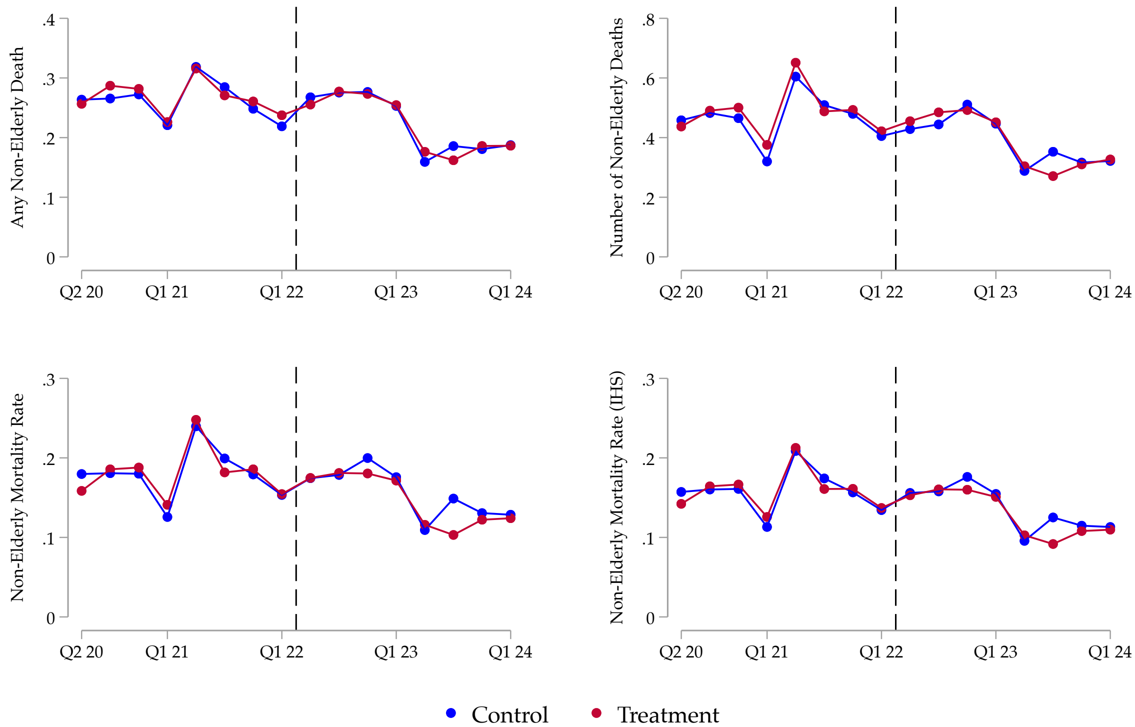
Notes: The figure shows weighted means for our all-age mortality outcomes for treatment and control group subcenters over time. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System.

Figure A7: Trends in Elderly Mortality Outcomes by Treatment Group



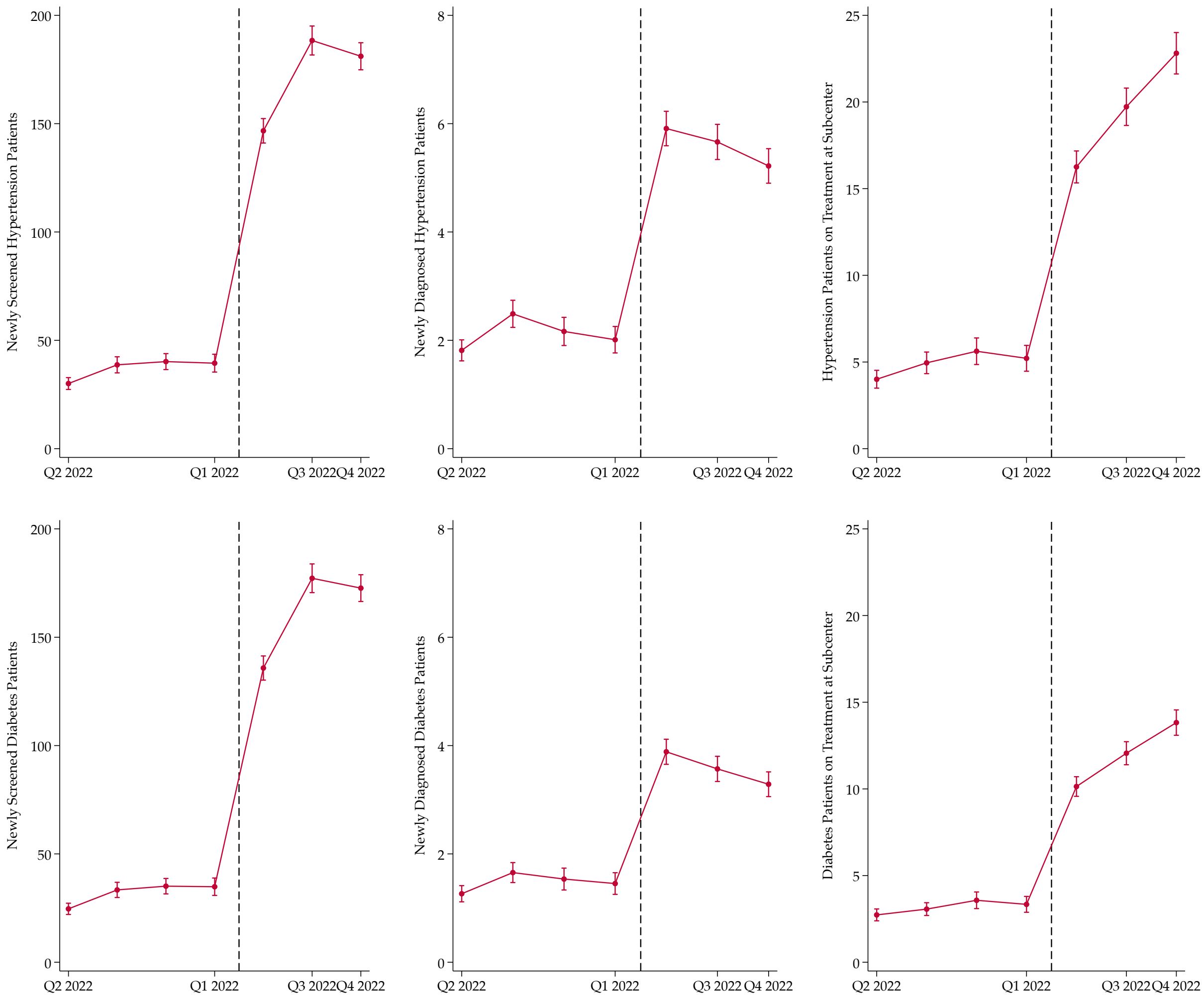
Notes: The figure shows weighted means for our elderly mortality outcomes for treatment and control group subcenters over time. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System.

Figure A8: Trends in Non-Elderly Mortality Outcomes by Treatment Group



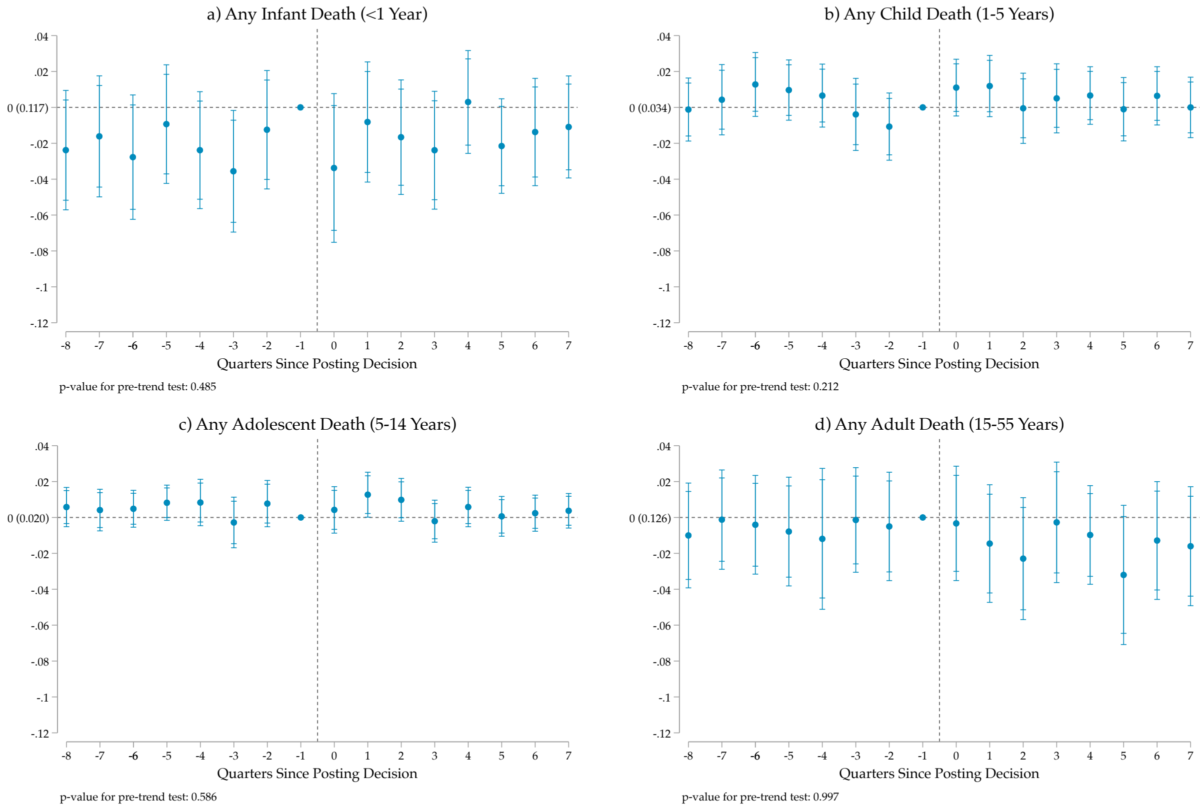
Notes: The figure shows weighted means for our non-elderly mortality outcomes for treatment and control group subcenters over time. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System.

Figure A9: Hypertension and Diabetes Patients at Treatment Group Subcenters Over Time



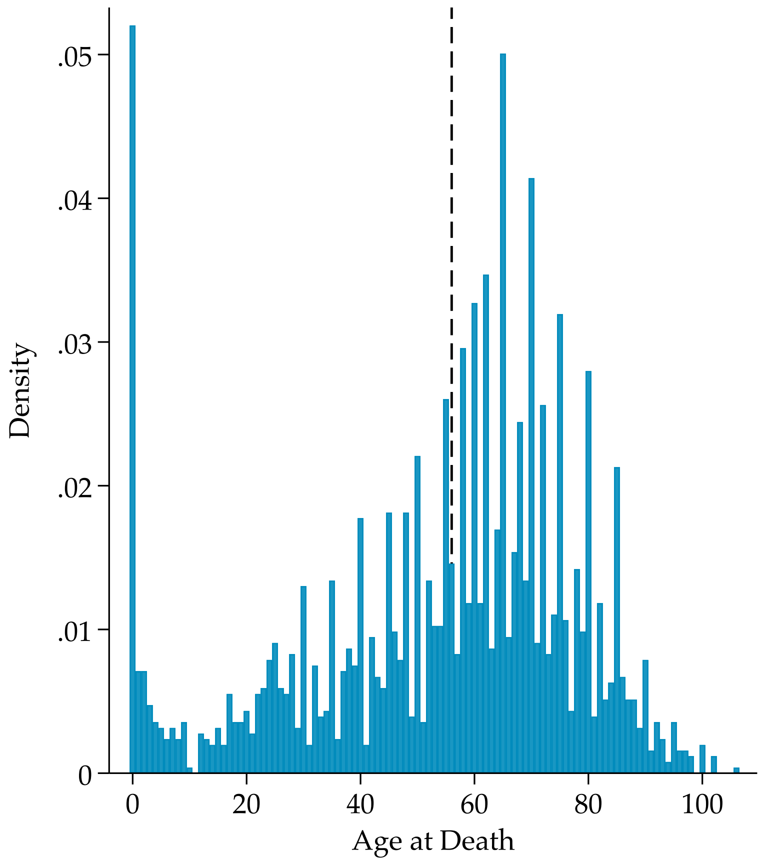
Notes: The figure shows the provision of chronic diseases services for treatment group subcenters over time. Information for control group subcenters areas not shown since they stopped reporting after the first cohort of CHOs was posted. The sample consists of 2,545 treatment group subcenters and the sample period covers Q2 2021 until Q4 2022. Outcomes are obtained from the Health and Wellness Center Portal.

Figure A10: Effects of Community Health Officers on Health Outcomes for Other Age Groups



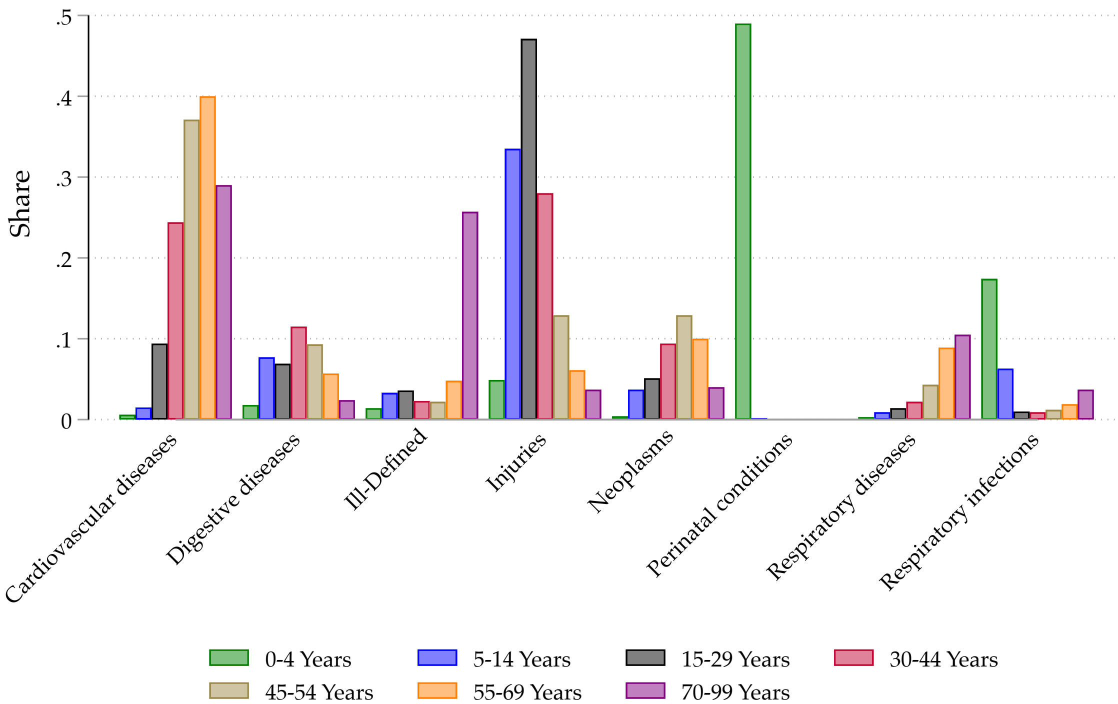
Notes: The figure shows weighted regression estimates of the effect of CHOs on health outcomes. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and quarter dummies, and year and subcenter fixed effects. The base quarter (Q1 2022) is omitted. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The figures show 95% confidence intervals based on standard errors clustered at the subcenter level. The number in parentheses on the y-axis shows the treatment group mean of the outcome in the base quarter. The p-value at the bottom of each figure corresponds to a chi-squared test of the null hypothesis that all interactions between treatment and pre-period dummies are statistically equal to zero. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System.

Figure A11: Distribution of Age at Death



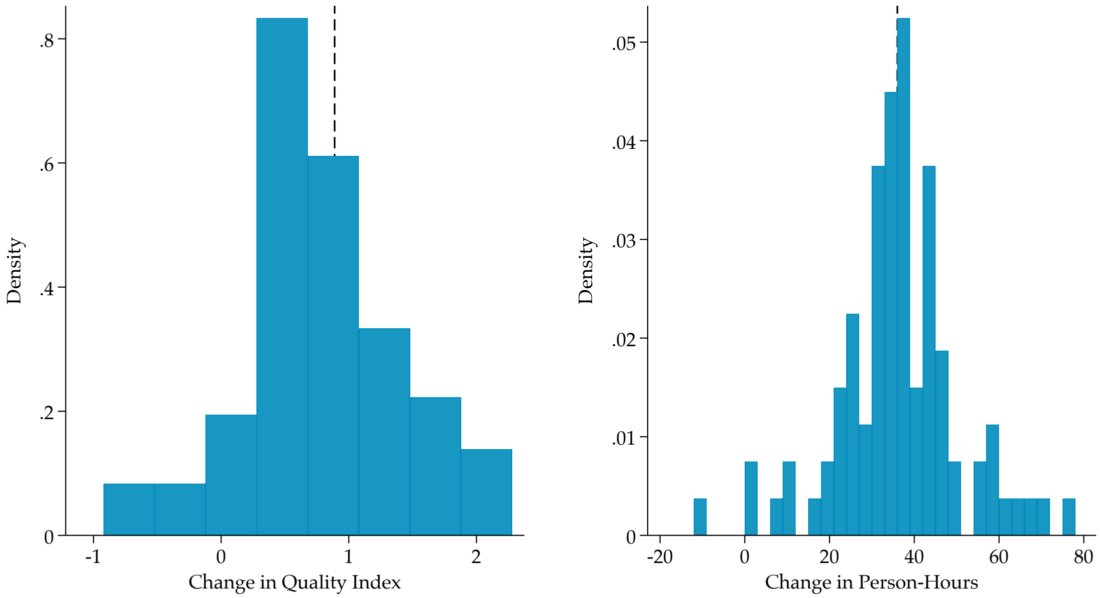
Notes: This sample reports the distribution of age at death using data from the 2017–2018 National Sample Survey. The dashed line indicates the 55-year cutoff.

Figure A12: Causes of Death by Age Group



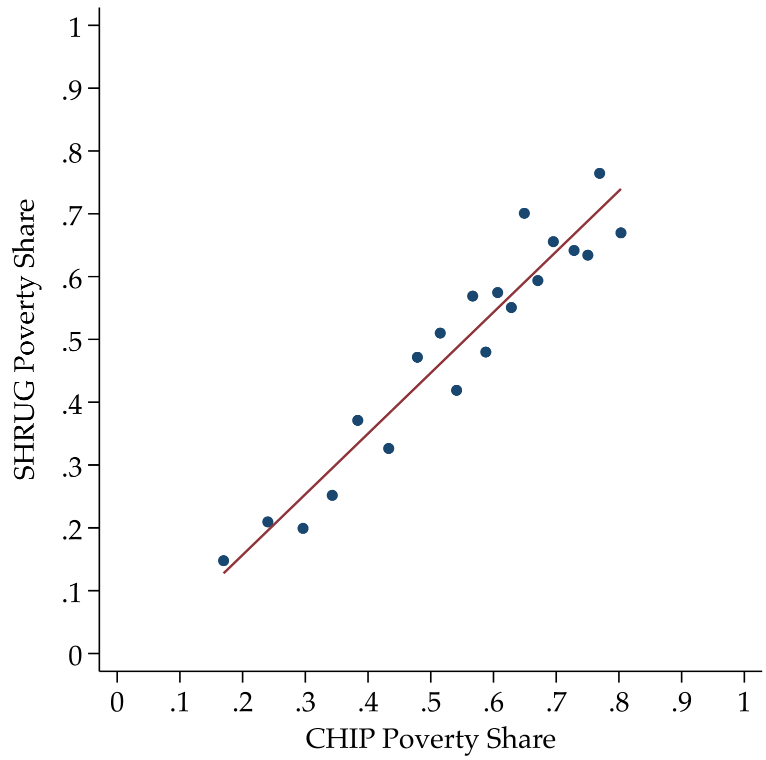
Notes: This sample reports the distribution of causes of death by age group using data from the official 'Causes of Death in India: 2017- 19' report based on data of the Sample Registration System.

Figure A13: Change in Subcenter Quality Index and Person-Hours



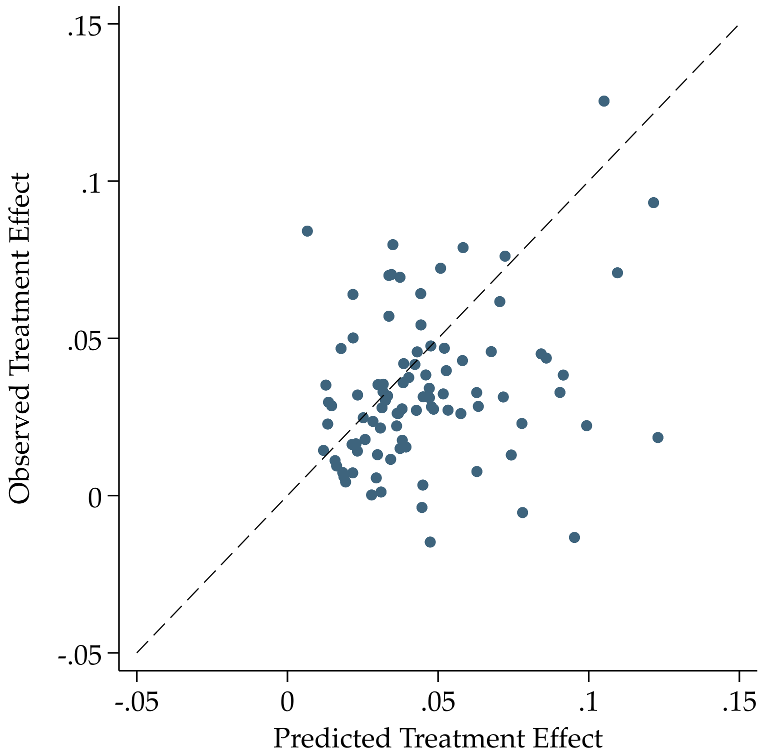
Notes: The figure shows the distribution of the change in quality index and person-hours between baseline and endline in treated subcenters. The dashed vertical lines indicate the median. Variables are obtained from provider surveys with CHOs and ANMs.

Figure A14: Village-Level Poverty Shares in SHRUG and CHIP data



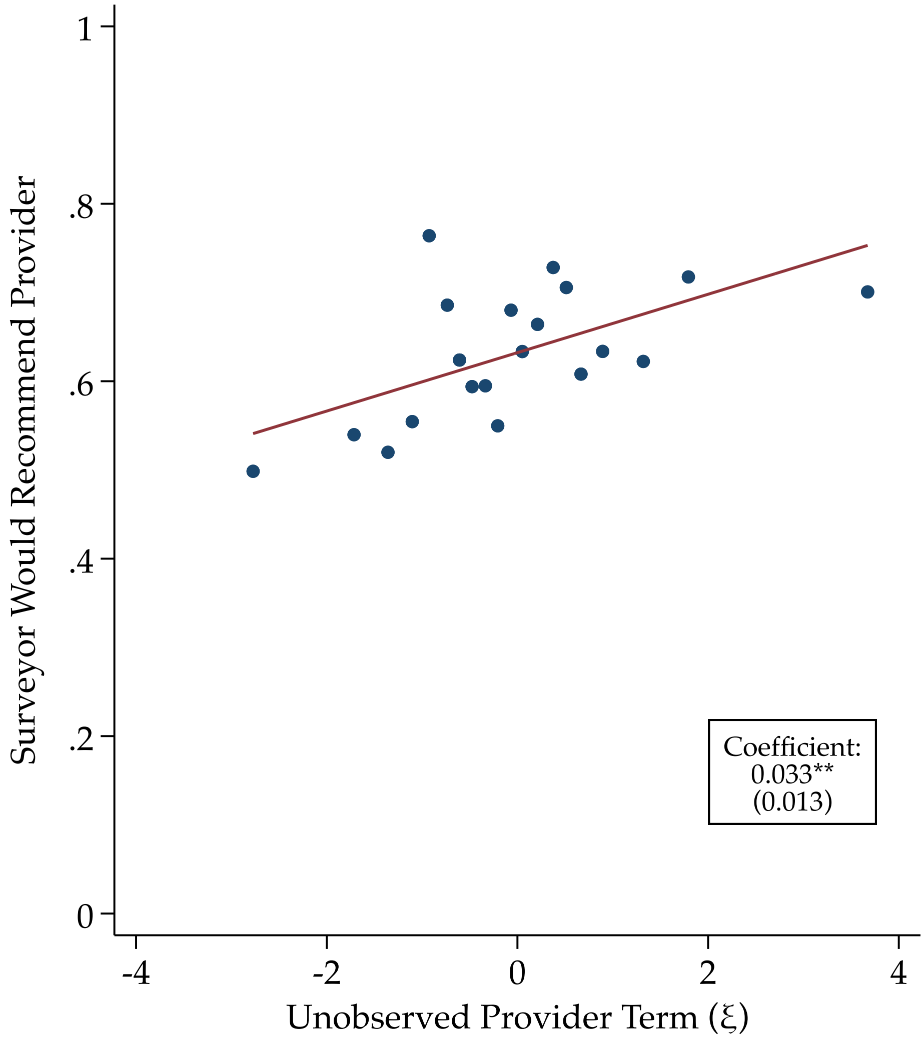
Notes: The figure is a binscatter plot of the imputed poverty shares in the SHRUG data against the imputed poverty shares in the CHIP data. SHRUG data generate imputed poverty shares by first regressing total household consumption on a set of asset and earnings information in the 2011-2012 Indian Human Development Survey data. They then combine these estimates with the asset and earnings information in the 2011 Socioeconomic and Caste Census microdata to predict household-level consumption (Asher et al., 2021). In the CHIP data, we define a household as poor if it meets fewer than three of the following conditions: (i) primary cooking fuel is kerosene or LPG, (ii) primary toilet has running water, (iii) primary drinking water comes from reverse osmosis system, tap water, or hand pump/tube well inside the house, (iv) household has electricity, (v) primary housing material is brick and concrete or wood, and (vi) primary transport is a motorcycle, car, tractor, or animal cart. The red line represents the fitted line from a linear regression.

Figure A15: Observed and Predicted Treatment Effects on Subcenter Market Shares



Notes: This figure plots the observed treatment effects on subcenter market shares against the predicted treatment effects by the demand model. The observed treatment effect come from the difference in patient visits between the pre- and post-period. The dashed line represents the 45-degree line.

Figure A16: Relationship Between Predicted Unobserved Provider Term and Surveyor Assessments



Notes: This figure is a binscatter plot of surveyor recommendation against the unobserved provider term (ξ_{jt}) that we estimate in the demand model. Surveyor recommendations are based on a binary question in our provider surveyors that asked surveyors whether they would recommend the provider to a friend after the survey was completed. The regression includes controls for provider quality, price, person-hours, electricity, and for whether the observation is a PHC or a private provider. The red line represents the fitted line from a linear regression.

Table A1: Incentive Payments

S. No.	Indicators	CHO	ASHA	ANM
1	Proportion of Pregnant Women registered who received ANC as per scheduled due date	1000	67	100
2	Proportion of new-borns who received Home Based Newborn Care services	1000	67	100
3a	Proportion of Children up to 1 years of age who received immunization as per the due date	500	33	50
3b	Proportion of Children up to 2 years of age who received immunization as per the due date	500	33	50
4	Proportion of cases referred for TB screening	1000	67	100
5	Number of footfalls in the month	1000	67	100
6	Proportion of individuals 30 years and above whose CBAC form was filled	1000	67	100
7a	Proportion of individual 30 years or above screened for Hypertension	500	33	50
7b	Proportion of Hypertension patients on treatment	500	33	50
8a	Proportion of individual 30 years or above screened for Diabetes	500	33	50
8b	Proportion of Diabetes patients on treatment	500	33	50
9	Teleconsultation Services	1000	67	100
10	Wellness sessions Organised at HWCs	1000	67	100
11	Wellness Activities held as per annual Health calendar	1000	67	100
12	Monthly JAS meeting held with minimum 60% of the members	1000	67	100
13	Village Meetings	1000	67	100
14	MCHN held against planned	1000	67	100
15a	Monitoring of Referral cases – Upward	500	33	50
15b	Monitoring Of Referral cases - Downward/ Follow up	500	33	50
		15000	**1000**	**1500**

Notes: The table shows the monthly incentive payments CHOs, community health workers (ASHAs), and ANMs receive for completing their targets. The payments are denoted in INR.

Table A2: Survey Completion Rates

	Public Providers		Private Providers		Households	
	Baseline Completion Rate (1)	Endline Completion Rate (2)	Baseline Completion Rate (3)	Endline Completion Rate (4)	Baseline Completion Rate (5)	Endline Completion Rate (6)
Treatment	-0.019	-0.020	-0.013	0.001	-0.012	-0.018
	(0.029)	(0.014)	(0.064)	(0.050)	(0.022)	(0.031)
Control Group Mean	0.968	1.000	0.711	0.882	0.267	0.910
Observations	193	193	280	169	1,971	513

Notes: This table shows differences in survey completion rates by treatment status. In each column, we regress a dummy for whether the survey was completed on a treatment dummy. The sample in Columns (1)-(2) consists of 193 ANMs. The sample in Column (3) consists of 280 private healthcare providers that we mapped across the 193 catchment areas. The sample in Column (4) consists of private providers that were surveyed at baseline and still operational at endline. The sample in Column (5) consists of households that have registered pregnancy in the past five years. The sample in Column (6) consists of all households that were surveyed at baseline. The endline survey was partly done in person.

Table A3: Comparison of Treatment and Control Areas in Survey Sample

	Original Sample				Reweighted Sample				
	Control Mean (1)	Control St. D. (2)	Treatment Coeff. (3)	Treatment St. E. (4)	Control Mean (5)	Control St. D. (6)	Treatment Coeff. (7)	Treatment St. E. (8)	N (9)
Panel A: Targeted Characteristics									
Distance to District HQ (in km)	67.09	[21.07]	-8.69***	(2.95)	57.21	[19.44]	1.18	(3.30)	193
Distance to Subdistrict HQ (in km)	23.03	[12.61]	-3.27*	(1.77)	18.72	[10.57]	1.05	(1.66)	193
Subcenter Has Electricity	0.69	[0.46]	-0.07	(0.07)	0.60	[0.49]	0.03	(0.09)	193
Subcenter Has Running Water	0.31	[0.46]	0.12*	(0.07)	0.39	[0.49]	0.04	(0.09)	193
Subcenter Building in Good Condition	0.31	[0.46]	0.16**	(0.07)	0.49	[0.50]	-0.03	(0.09)	193
Panel B: Catchment Area Characteristics									
Distance to Public Health Clinic (in km)	6.90	[5.31]	-0.36	(0.87)	7.66	[6.93]	-1.12	(1.28)	193
Total Population	3082.30	[1254.20]	266.88	(189.28)	3190.21	[1364.64]	158.97	(257.48)	193
Scheduled Caste Share	0.06	[0.05]	0.01*	(0.01)	0.07	[0.05]	0.00	(0.01)	185
Scheduled Tribe Share	0.46	[0.32]	-0.07	(0.05)	0.36	[0.29]	0.04	(0.05)	185
Female Share	0.49	[0.01]	-0.00	(0.00)	0.49	[0.01]	-0.00	(0.00)	185
Literacy Rate	0.47	[0.08]	0.01	(0.01)	0.49	[0.07]	-0.01	(0.01)	185
Land Ownership Rate	0.68	[0.14]	-0.00	(0.02)	0.70	[0.12]	-0.02	(0.02)	184
Employment Rate	0.50	[0.09]	-0.01	(0.01)	0.50	[0.09]	-0.02	(0.02)	185
(Imputed) Consumption per Capita (in INR)	15264.34	[3352.43]	766.40	(482.08)	16417.50	[3236.80]	-386.75	(559.93)	184
Number of Covid-19 Cases	15.94	[29.05]	-0.78	(3.79)	15.14	[24.98]	0.02	(3.34)	172
Number of Covid-19 Deaths	0.64	[1.14]	0.22	(0.18)	0.63	[1.16]	0.22	(0.20)	175
Panel C: Average Facility Indicators in Q1 2022									
Number of Patients	208.82	[145.82]	19.65	(19.62)	256.54	[195.29]	-28.06	(34.89)	188
Number of Acute Heart Disease Patients	0.04	[0.25]	-0.04*	(0.03)	0.01	[0.13]	-0.01*	(0.01)	188
Number of Hypertension Patients	3.43	[6.87]	-0.18	(1.03)	3.35	[6.35]	-0.10	(1.16)	188
Maternal and Child Health Services Index	0.00	[0.78]	0.15	(0.12)	0.01	[0.81]	0.14	(0.16)	188
All-Age Mortality Rate	0.24	[0.49]	0.03	(0.07)	0.32	[0.66]	-0.06	(0.14)	188
Elderly Mortality Rate	0.84	[2.57]	0.52	(0.40)	0.88	[2.42]	0.48	(0.47)	188

Notes: This table shows the means of selected covariates for the original and reweighted survey sample. Panel A reports on the covariates used to estimate the propensity score for the inverse probability weighting. Panel B reports on covariates at the catchment area level. Catchment-level covariates are calculated based on population-weighted averages across all villages in the catchment area of the respective subcenter. The number of Covid-19 cases and deaths come from the ANM baseline survey. Panel C reports on the main outcomes in the pre-treatment reference period. Columns (1)-(4) present the original sample and Columns (5)-(8) present the reweighted sample. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. Differences in sample sizes across variables reflect missing data. Columns (1) and (5) report the control mean of the dependent variable for each relevant sample. Columns (3), and (7) report the difference in the dependent variable from OLS regressions of each outcome on an indicator variable for CHO assignment.

Table A4: Effects of Community Health Officers on Subcenter Quality Index Components

	Quality Index Components						
	Length of Medical Degree	Checklist Completion Rate (Asthma Vignette)	Correct Treatment / Referral (Asthma Vignette)	Antibiotics Given (Asthma Vignette)	Checklist Completion Rate (Child Dysentery Vignette)	Correct Treatment / Referral (Child Dysentery Vignette)	Antibiotics Given (Child Dysentery Vignette)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment × Post	1.301***	0.896***	-0.027	0.071	0.383	0.029	0.182*
	(0.058)	(0.255)	(0.087)	(0.079)	(0.269)	(0.031)	(0.102)
Control Group Mean (Baseline)	2.067	1.577	0.665	0.227	1.453	0.987	0.345
Treatment Group Mean (Baseline)	2.096	1.194	0.750	0.076	1.341	0.934	0.451
Counterfactual Treatment Group Mean (Endline)	2.082	0.874	0.975	-0.029	0.722	0.939	0.381
Observations	378	371	369	369	365	366	366

Notes: This table shows the effects of CHOs on subcenter quality index components and child dysentery vignette performance. We regress the outcome on the treatment dummy, the post-period dummy, and an interaction between both of them. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. The sample consists of 193 subcenters and outcomes are obtained from ANM and CHO surveys.

Table A5: Results from Patient Exit Surveys at Endline

	Overall Satisfaction	Number of Questions Asked	Measured Blood Pressure	Any Antibiotics	Referred
	(1)	(2)	(3)	(4)	(5)
Treatment	0.376**	0.545**	0.231***	0.051	0.085***
	(0.180)	(0.209)	(0.070)	(0.077)	(0.031)
Mean of Outcome	4.108	1.725	0.066	0.105	0.010
Observations	172	173	177	177	174

Notes: This table shows the effects of CHOs on healthcare quality according to patient exit survey. In each column, we regress the outcome on an indicator variable for whether a CHO was assigned to the subcenter in March 2022. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. The sample consists of 177 patients who visited the subcenter for outpatient care services. The satisfaction outcome ranges from 1 (lowest) to 5 (highest). See Data Appendix for details on variable definitions.

Table A6: Differences in Additional Patient Visits Between Treatment and Control Group Subcenters

	Number of Patient Visits					
	Eye (1)	Oral (2)	Mental (3)	Pallative (4)	COPD (5)	Asthma (6)
Treatment	2.752***	1.604***	0.061**	0.162***	0.149***	0.366***
	(0.434)	(0.310)	(0.026)	(0.026)	(0.033)	(0.079)
Control Group Mean	9.561	6.179	0.180	0.101	0.156	0.599
Observations	19,493	19,493	19,493	19,493	19,493	19,493

Notes: This table shows differences in patient visits for six patient types between treatment and control group subcenter in the post-period. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2023 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table A7: Effects of Community Health Officers on Maternal and Child Health Services Index Components

		Maternal & Child Health Services Index Components				
		N Pregnant Women				
	N Pregnant Women Registered	4+ Prenatal Care Visits	360 Calcium Tablets	1st Tetanus Shot	N Children Fully Immunized	N Postnatal Care Visits
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Post	0.038	0.067	-0.364*	0.005	0.052	0.131
	(0.133)	(0.142)	(0.214)	(0.100)	(0.111)	(0.133)
Control Group Mean (Pre-Periods)	16.640	10.319	10.908	9.942	14.490	8.923
Treatment Group Mean (Pre-Periods)	16.869	10.630	11.371	10.178	14.744	9.284
Counterfactual Treatment Group Mean (Post-Periods)	16.406	12.058	13.462	10.308	14.544	10.364
Observations	9,818	9,818	9,818	9,818	9,818	9,818

Notes: This table shows the effects of CHOs on the components of the maternal and child health services index. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table A8: Effects of Community Health Officers on Mortality Outcomes for Other Age Groups

	Deaths by Age Group			
	Infant (1 < Year) (1)	Child (1-4 Years) (2)	Adolescent (5-14 Years) (3)	Adult (15-55 Years) (4)
Panel A: Any Death				
Treatment × Post	0.003	0.003	0.000	-0.009
	(0.007)	(0.003)	(0.002)	(0.009)
Control Group Mean (Pre-Periods)	0.141	0.035	0.016	0.136
Treatment Group Mean (Pre-Periods)	0.138	0.034	0.017	0.144
Counterfactual Treatment Group Mean (Post-Periods)	0.107	0.026	0.013	0.127
Observations	9,812	9,812	9,812	9,812
Panel B: Mortality Rate (IHS)				
Treatment × Post	0.008	0.010	0.000	-0.012
	(0.173)	(0.010)	(0.002)	(0.009)
Control Group Mean (Pre-Periods)	2.902	0.115	0.015	0.133
Treatment Group Mean (Pre-Periods)	2.814	0.109	0.016	0.139
Counterfactual Treatment Group Mean (Post-Periods)	2.232	0.086	0.012	0.123
Observations	9,812	9,812	9,812	9,812

Notes: This table shows the effects of CHOs on mortality outcomes for different age groups. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table A9: Effects of Community Health Officers on Elderly Mortality by Causes of Death

	Elderly (56+ Years) Deaths			
	Chronic	Acute	Accident	Unknown Cause
	(1)	(2)	(3)	(4)
Panel A: Any Death				
Treatment × Post	-0.009	0.001	0.002	-0.038***
	(0.007)	(0.004)	(0.002)	(0.009)
Control Group Mean (Pre-Periods)	0.097	0.043	0.009	0.153
Treatment Group Mean (Pre-Periods)	0.103	0.045	0.011	0.167
Counterfactual Treatment Group Mean (Post-Periods)	0.090	0.041	0.011	0.196
Observations	9,818	9,818	9,818	9,818
Panel B: Mortality Rate (IHS)				
Treatment × Post	-0.015	0.001	0.003	-0.104***
	(0.017)	(0.010)	(0.003)	(0.027)
Control Group Mean (Pre-Periods)	0.243	0.097	0.014	0.422
Treatment Group Mean (Pre-Periods)	0.251	0.104	0.018	0.449
Counterfactual Treatment Group Mean (Post-Periods)	0.212	0.095	0.018	0.528
Observations	9,818	9,818	9,818	9,818

Notes: This table shows the effects of CHOs on elderly mortality for different causes of death. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table A10: Robustness Checks Related to Reporting Bias

	At Least One Pre-Period Elderly Death (PCTS)				Civil Registration System
	Any Elderly Death	Number of Elderly Deaths	Elderly Mortality Rate	Elderly Mortality Rate (IHS)	Number of Elderly Deaths
	(1)	(2)	(3)	(4)	(5)
Treatment × Post	-0.057***	-0.150**	-0.582**	-0.167***	-0.123*
	(0.019)	(0.064)	(0.284)	(0.057)	(0.074)
Control Group Mean (Pre-Periods)	0.469	1.225	5.485	1.360	6.214
Treatment Group Mean (Pre-Periods)	0.490	1.252	5.272	1.387	6.629
Counterfactual Treatment Group Mean (Post-Periods)	0.465	1.180	4.942	1.322	4.676
Observations	5,018	5,018	5,018	5,018	7,670

Notes: This table addresses concerns related to reporting bias in administrative data. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. In Columns (1)-(4), the sample consists of 2,509 subcenters that at least one death in the pre-period. The sample period covers Q2 2020 until Q1 2024 and outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. In Column (5), the sample consists of 3,835 gram panchayats and the sample period covers Q2 2021 until Q1 2023. The outcome is obtained from the Civil Registration System and the treatment dummy is equal to one if at least half of the subcenters in a gram panchayat received a CHO in March 2022. See Data Appendix for details on variable definitions.

Table A11: Effects of Community Health Officers on Hospitalizations by Age Group

	Any Hospitalization			
	Elderly (1)	Non-Elderly (2)	Poor (3)	Non-Poor (4)
Treatment × Post	-0.044**	-0.010	-0.008	-0.021
	(0.022)	(0.009)	(0.014)	(0.013)
Control Group Mean (Baseline)	0.010	0.027	0.024	0.024
Treatment Group Mean (Baseline)	0.042	0.035	0.034	0.038
Counterfactual Treatment Group Mean (Endline)	0.073	0.023	0.026	0.037
Observations	1,160	4,687	1,943	3,246

Notes: This table shows the effects of CHOs on hospitalizations at the household member level by age group and poverty status. The regression coefficients are estimated by regressing the outcome on the treatment dummy, the post-period dummy, and an interaction between both of them. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. Outcomes are obtained from our household surveys. See Data Appendix for details on variable definitions.

Table A12: Robustness Regarding Common Support Restrictions

	Number of Patient Visits	Number of Hypertension Patient Visits	Number of Acute Heart Disease Patient Visits	Maternal & Child Health Services Index	Any Death	All-Age Mortality Rate (IHS)	Any Elderly Death	Elderly Mortality Rate (IHS)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Districts with 10-90% Assignment Rate, Minimal Common Support Restriction								
Treatment × Post	220.379***	6.602***	0.027***	-0.002	-0.021**	-0.024**	-0.026***	-0.077***
	(7.657)	(0.556)	(0.006)	(0.011)	(0.010)	(0.011)	(0.009)	(0.028)
Observations	10,592	10,592	10,592	10,592	10,592	10,592	10,592	10,592
Panel B: Districts with 10-90% Assignment Rate , 1% Common Support Restriction								
Treatment × Post	220.996***	6.574***	0.026***	-0.000	-0.021**	-0.024**	-0.026***	-0.077***
	(7.678)	(0.564)	(0.006)	(0.011)	(0.010)	(0.011)	(0.009)	(0.028)
Observations	10,388	10,388	10,388	10,388	10,388	10,388	10,388	10,388
Panel C: Districts with 10-90% Assignment Rate, 5% Common Support Restriction								
Treatment × Post	213.428***	5.950***	0.026***	-0.001	-0.024**	-0.027**	-0.031***	-0.091***
	(7.957)	(0.603)	(0.006)	(0.011)	(0.011)	(0.012)	(0.010)	(0.030)
Observations	9,186	9,186	9,186	9,186	9,186	9,186	9,186	9,186
Panel D: Districts with 15-85% Assignment Rate, Minimal Common Support Restriction								
Treatment × Post	225.371***	6.729***	0.026***	-0.002	-0.019*	-0.024**	-0.026***	-0.080***
	(8.145)	(0.617)	(0.006)	(0.011)	(0.011)	(0.012)	(0.009)	(0.028)
Observations	9,138	9,138	9,138	9,138	9,138	9,138	9,138	9,138
Panel E: Districts with 15-85% Assignment Rate, 1% Common Support Restriction								
Treatment × Post	226.487***	6.670***	0.025***	-0.001	-0.019*	-0.023**	-0.026***	-0.079***
	(8.174)	(0.625)	(0.007)	(0.011)	(0.011)	(0.011)	(0.009)	(0.028)
Observations	8,936	8,936	8,936	8,936	8,936	8,936	8,936	8,936
Panel F: Districts with 15-85% Assignment Rate, 2.5% Common Support Restriction								
Treatment × Post	226.487***	6.670***	0.025***	-0.001	-0.019*	-0.023**	-0.026***	-0.079***
	(8.174)	(0.625)	(0.007)	(0.011)	(0.011)	(0.011)	(0.009)	(0.028)
Observations	8,936	8,936	8,936	8,936	8,936	8,936	8,936	8,936
Panel G: Districts with 15-85% Assignment Rate, 5% Common Support Restriction								
Treatment × Post	217.625***	5.946***	0.026***	-0.000	-0.021*	-0.024**	-0.033***	-0.095***
	(8.593)	(0.668)	(0.007)	(0.012)	(0.012)	(0.012)	(0.010)	(0.031)
Observations	7,850	7,850	7,850	7,850	7,850	7,850	7,850	7,850

Notes: This table shows the effects of CHOs on healthcare services and health outcomes for different sample restrictions. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table A13: Effects of Community Health Officers on Alternative Top-Coding Strategies

	All-Age Mortality			Elderly (56+) Mortality		
	Number of Deaths (1)	Mortality Rate (2)	Mortality Rate (IHS) (3)	Number of Deaths (4)	Mortality Rate (5)	Mortality Rate (IHS) (6)
Panel A: No Top-Coding						
Treatment × Post	-0.148**	-0.314	-0.030*	-0.131***	-4.427	-0.096***
	(0.059)	(0.677)	(0.017)	(0.049)	(7.425)	(0.033)
Control Group Mean (Pre-Periods)	1.122	1.655	0.330	0.638	13.924	0.678
Treatment Group Mean (Pre-Periods)	1.232	0.938	0.325	0.723	6.951	0.705
Counterfactual Treatment Group Mean (Post-Periods)	1.131	1.481	0.318	0.702	13.388	0.739
Observations	9,818	9,818	9,818	9,818	9,818	9,818
Panel C: Top-Coding 95%						
Treatment × Post	-0.107***	-0.037**	-0.030***	-0.081***	-0.300***	-0.093***
	(0.033)	(0.015)	(0.011)	(0.026)	(0.108)	(0.028)
Control Group Mean (Pre-Periods)	0.921	0.357	0.292	0.517	2.178	0.637
Treatment Group Mean (Pre-Periods)	0.976	0.360	0.297	0.557	2.236	0.675
Counterfactual Treatment Group Mean (Post-Periods)	0.958	0.354	0.293	0.582	2.341	0.709
Observations	9,818	9,818	9,818	9,818	9,818	9,818

Notes: This table shows the effects of CHOs on healthcare services and health outcomes for alternative top-coding strategies. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. The sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table A14: Alternative Weighting

	Number of Patient Visits	Number of Hypertension Patient Visits	Number of Acute Heart Disease Patient Visits	Maternal & Child Health Services Index	Any Death	All-Age Mortality Rate (IHS)	Any Elderly Death	Elderly Mortality Rate (IHS)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Propensity Scores using LASSO								
ATET								
Treatment × Post	216.930***	6.331***	0.033***	0.005	-0.023**	-0.022**	-0.025***	-0.069**
	(9.179)	(0.586)	(0.009)	(0.014)	(0.010)	(0.011)	(0.009)	(0.027)
Control Group Mean (Pre-Periods)	249.387	3.781	0.044	0.087	0.369	0.319	0.237	0.688
Treatment Group Mean (Pre-Periods)	239.272	3.470	0.038	0.033	0.372	0.314	0.246	0.695
Counterfactual Treatment Group Mean (Post-Periods)	369.434	8.527	0.028	0.139	0.356	0.297	0.249	0.701
Observations	9,818	9,818	9,818	9,818	9,818	9,818	9,818	9,818
Panel B: Entropy Balancing								
ATET								
Treatment × Post	223.433***	6.496***	0.027***	0.000	-0.016	-0.017	-0.020**	-0.057**
	(8.270)	(0.550)	(0.007)	(0.012)	(0.010)	(0.011)	(0.009)	(0.026)
Control Group Mean (Pre-Periods)	238.536	3.896	0.041	0.000	0.361	0.314	0.230	0.670
Treatment Group Mean (Pre-Periods)	239.272	3.470	0.038	0.033	0.372	0.314	0.246	0.695
Counterfactual Treatment Group Mean (Post-Periods)	362.930	8.361	0.034	0.143	0.349	0.292	0.245	0.689
Observations	9,818	9,818	9,818	9,818	9,818	9,818	9,818	9,818

Notes: This table shows the effects of CHOs on healthcare services and health outcomes for alternative weighting strategies. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. In Panel A, subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores estimated based on a lasso regression that uses all the covariates listed in Table 1. In Panel B, subcenter-level weights for the control group are constructed by entropy balancing using the first-order moments of the following variables: distance to the district HQ, distance to the subdistrict HQ, and district fixed effects. The sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table A15: Alternative Difference-in-Differences Estimators

	Number of Patient Visits	Number of Hypertension Patient Visits	Number of Acute Heart Disease Patient Visits	Maternal & Child Health Services Index	Any Death	All-Age Mortality Rate (IHS)	Any Elderly Death	Elderly Mortality Rate (IHS)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Subcenter-Specific Linear Trends								
Treatment × Post	205.863***	3.570***	0.007	0.004	-0.031	-0.028	-0.030**	-0.091*
	(7.810)	(0.427)	(0.011)	(0.015)	(0.019)	(0.020)	(0.015)	(0.047)
Control Group Mean (Pre-Periods)	237.503	3.731	0.036	-0.012	0.364	0.313	0.229	0.665
Treatment Group Mean (Pre-Periods)	239.272	3.470	0.038	0.033	0.372	0.314	0.246	0.695
Counterfactual Treatment Group Mean (Post-Periods)	380.501	11.287	0.054	0.140	0.364	0.303	0.255	0.723
Observations	78,544	78,544	78,401	78,544	78,544	78,544	78,544	78,544
Panel B: Double-Robust Estimator								
ATET								
Treatment × Post	214.849***	6.176***	0.026***	-0.003	-0.027***	-0.029***	-0.031***	-0.088***
	(7.777)	(0.571)	(0.007)	(0.012)	(0.010)	(0.011)	(0.009)	(0.027)
Control Group Mean (Pre-Periods)	237.503	3.731	0.036	-0.012	0.364	0.313	0.229	0.665
Treatment Group Mean (Pre-Periods)	239.272	3.470	0.038	0.033	0.372	0.314	0.246	0.695
Counterfactual Treatment Group Mean (Post-Periods)	371.514	8.681	0.035	0.147	0.360	0.304	0.255	0.721
Observations	9,818	9,818	9,818	9,818	9,818	9,818	9,818	9,818
Panel C: Removing Covid-19 Quarters								
ATET								
Treatment × Post	217.684***	6.120***	0.024***	0.001	-0.021**	-0.018	-0.022**	-0.058**
	(7.723)	(0.576)	(0.007)	(0.012)	(0.011)	(0.011)	(0.009)	(0.027)
Control Group Mean (Pre-Periods)	246.651	4.312	0.034	0.043	0.358	0.304	0.228	0.652
Treatment Group Mean (Pre-Periods)	245.731	4.113	0.038	0.084	0.362	0.294	0.236	0.653
Counterfactual Treatment Group Mean (Post-Periods)	368.680	8.737	0.037	0.142	0.355	0.293	0.246	0.691
Observations	9,818	9,818	9,818	9,818	9,818	9,818	9,818	9,818
Panel D: Staggered Difference-in-Differences Estimator								
ATT	235.787***	6.265***	0.019***	0.003	-0.028**	-0.031**	-0.035***	-0.100***
	(8.805)	(0.511)	(0.007)	(0.015)	(0.013)	(0.014)	(0.012)	(0.033)
Observations	18,212	18,212	18,212	18,212	18,212	18,212	18,212	18,212

Notes: This table shows the effects of CHOs on healthcare services and health outcomes for alternative difference-in-differences estimators. In Panel A, the regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy, year and subcenter fixed effects, and an interaction between linear time trends and subcenter fixed effects. In Panel B, the regression coefficients are estimated using the double robust estimator proposed by Sant'Anna and Zhao (2020). In Panel C, we remove all observations before Q3 2021 from the sample. In Panel D, we also account for also control group subcenters that received a CHO in December 2022 using the estimator proposed by Callaway and Sant'Anna (2021). The sample in Panel A consists of subcenter-quarter observations and the sample in Panels B and C is aggregated at the subcenters-pre/post level. The sample in Panel D is aggregated into three periods (no subcenter is treated, first cohort of CHOs is assigned, second cohort of CHOs is assigned). Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table A16: Alternative Empirical Strategy Based on Closest Subcenter

	Number of Patient Visits	Number of Hypertension Patient Visits	Number of Acute Heart Disease Patient Visits	Maternal & Child Health Services Index	Any Death	All-Age Mortality Rate (IHS)	Any Elderly Death	Elderly Mortality Rate (IHS)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post	225.546***	5.980***	0.040***	0.005	-0.034**	-0.036**	-0.032**	-0.094**
	(9.851)	(0.863)	(0.008)	(0.016)	(0.016)	(0.018)	(0.014)	(0.043)
Control Group Mean (Pre-Periods)	249.683	3.703	0.042	-0.067	0.370	0.328	0.243	0.710
Treatment Group Mean (Pre-Periods)	239.207	3.433	0.040	0.038	0.372	0.315	0.246	0.701
Counterfactual Treatment Group Mean (Post-Periods)	362.791	9.372	0.022	0.146	0.371	0.315	0.259	0.738
Observations	11,020	11,020	11,020	11,020	11,020	11,020	11,020	11,020

Notes: This table shows the effects of CHOs on healthcare services and health outcomes. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effects. Standard errors are clustered at the subcenter level. Each treatment group subcenter is matched to the closest control group subcenters (with replacement). The sample consists of 5,510 subcenters and the sample period covers Q2 2020 until Q1 2024. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

Table A17: Heterogeneity by CHO and ANM Baseline Quality

	Number of Patient Visits (1)	Any Death (2)	All-Age Mortality Rate (3)	All-Age Mortality Rate (IHS) (4)	Number of Patient Visits (5)	Any Death (6)	All-Age Mortality Rate (7)	All-Age Mortality Rate (IHS) (8)
Treatment × Post × Δ CHO Quality Index ≥ Median	279.565***	-0.111**	-0.092	-0.078				
	(26.811)	(0.050)	(0.064)	(0.048)				
Treatment × Post × Δ CHO Quality Index < Median	245.169***	-0.068	-0.014	-0.019				
	(29.087)	(0.055)	(0.068)	(0.052)				
Treatment × Post × Δ Base. ANM Quality Index ≥ Median					283.545***	-0.104	-0.005	-0.019
					(27.089)	(0.070)	(0.103)	(0.076)
Treatment × Post × Δ Base. ANM Quality Index < Median					236.351***	-0.046	-0.085*	-0.063
					(35.406)	(0.052)	(0.050)	(0.040)
p-value: Coef 1 = Coef 2	0.311	0.426	0.162	0.184	0.291	0.504	0.485	0.604
Treatment (≥ Median) :								
Treatment Group Mean (Pre-Periods)	265.733	0.409	0.343	0.286	253.452	0.375	0.294	0.248
Counterfactual Treatment Group Mean (Post-Periods)	309.075	0.370	0.284	0.242	268.740	0.344	0.184	0.172
Observations	118	118	118	118	94	94	94	94
Treatment (< Median) :								
Treatment Group Mean (Pre-Periods)	211.775	0.300	0.219	0.188	236.150	0.342	0.286	0.239
Counterfactual Treatment Group Mean (Post-Periods)	255.117	0.261	0.160	0.144	300.096	0.276	0.261	0.212
Observations	70	70	70	70	90	90	90	90
Control Group:								
Control Group Mean (Pre-Periods)	240.661	0.349	0.288	0.241	240.295	0.344	0.285	0.238
Observations	182	182	182	182	182	182	182	182

Notes: This table shows the effects of CHOs on healthcare services and health outcomes by CHO and ANM baseline quality. In Columns (1)-(4), the regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and a dummy for whether the CHO's quality index is equal to or larger than the median, and year and subcenter fixed effects. In Columns (5)-(8), the regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and year and subcenter fixed effect, separately by whether the ANM baseline quality index equal to or larger than the median. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. Outcomes are obtained from the Pregnancy, Child Tracking and Health Services Management System, and changes in the quality index are based on surveys with ANMs and CHOs. See Data Appendix for details on variable definitions.

Table A18: Effects of Community Health Officers on Private Provider Quality Index Components

	Quality Index Components				
	Length of Medical Degree	Checklist Completion Rate (Asthma Vignette)	Correct Treatment / Referral (Asthma Vignette)	Any Death	All-Age Mortality Rate (IHS)
	(1)	(2)	(3)		
Panel A: Pooled					
Treatment × Post	0.437*	0.001	0.147		
	(0.263)	(0.012)	(0.102)		
Control Group Mean (Baseline)	2.802	0.061	0.651		
Treatment Group Mean (Baseline)	2.448	0.065	0.512		
Counterfactual Treatment Group Mean (Endline)	2.124	0.056	0.496		
Observations	361	354	351		
Panel B: Heterogeneity by Number of Providers at Baseline					
Treatment × Post × 1 Private Provider at Baseline	0.983**	0.021	0.333**	-0.133	-0.112
	(0.488)	(0.021)	(0.155)	(0.102)	(0.109)
Treatment × Post × ≥ 2 Private Providers at Baseline	0.141	-0.008	0.026	-0.054	0.013
	(0.282)	(0.016)	(0.130)	(0.068)	(0.066)
p-value: Coef 1 = Coef 2	0.138	0.271	0.131	0.524	0.330
Only One Private Provider at Baseline :					
Control Group Mean (Baseline)	2.357	0.065	0.747	0.309	0.250
Treatment Group Mean (Baseline)	2.438	0.058	0.500	0.405	0.288
Counterfactual Treatment Group Mean (Endline)	1.627	0.038	0.423	0.376	0.265
Observations	123	120	120	126	126
At Least Two Private Provider at Baseline:					
Control Group Mean (Baseline)	2.947	0.059	0.617	0.322	0.207
Treatment Group Mean (Baseline)	2.455	0.069	0.519	0.357	0.225
Counterfactual Treatment Group Mean (Endline)	2.433	0.065	0.529	0.313	0.148
Observations	231	227	224	110	110

Notes: This table shows the effects of CHOs on the components of the private provider quality index. In Panel A, we regress the outcome on the treatment dummy, survey round dummies, and an interaction between the treatment dummy and the post-period dummy. In Panel B, we regress the outcome on the treatment dummy, the post-survey dummies, and an interaction between the treatment dummy and the post-period dummy, separately for catchment areas with only one or at least two private providers at baseline. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. Outcomes are obtained from our private provider surveys in Columns (1)-(3) and from the Pregnancy, Child Tracking and Health Services Management System in Columns (4)-(5). See Data Appendix for details on variable definitions.

Table A19: Effects of Community Health Officers on Private Provider Medications

	Injection Given (Asthma Vignette) (1)	Antibiotics Given (Asthma Vignette) (2)	Injection Rate (3)	Antibiotic Dispensing Rate (4)
Panel A: Pooled				
Treatment × Post	-0.026	0.040	-0.019	0.043
	(0.082)	(0.062)	(0.079)	(0.083)
Control Group Mean (Baseline)	0.210	0.117	0.189	0.366
Treatment Group Mean (Baseline)	0.232	0.073	0.208	0.336
Counterfactual Treatment Group Mean (Endline)	0.312	0.073	0.272	0.331
Observations	353	351	256	255
Panel B: Heterogeneity by Number of Providers at Baseline				
Treatment × Post × 1 Private Provider at Baseline	-0.210	-0.003	-0.115	0.016
	(0.134)	(0.130)	(0.091)	(0.110)
Treatment × Post × $\geq$ 2 Private Providers at Baseline	0.075	0.072	0.000	0.006
	(0.107)	(0.072)	(0.096)	(0.103)
p-value: Coef 1 = Coef 2	0.100	0.618	0.384	0.948
Only One Private Provider at Baseline :				
Control Group Mean (Baseline)	0.097	0.149	0.111	0.237
Treatment Group Mean (Baseline)	0.267	0.067	0.243	0.312
Counterfactual Treatment Group Mean (Endline)	0.429	0.076	0.388	0.346
Observations	120	120	90	91
At Least Two Private Provider at Baseline:				
Control Group Mean (Baseline)	0.249	0.105	0.203	0.388
Treatment Group Mean (Baseline)	0.212	0.077	0.184	0.352
Counterfactual Treatment Group Mean (Endline)	0.277	0.076	0.243	0.366
Observations	226	224	159	157

Notes: This table shows the effects of CHOs on the components of the private provider quality index. In Panel A, we regress the outcome on the treatment dummy, survey round dummies, and an interaction between the treatment dummy and the post-period dummy. In Panel B, we regress the outcome on the treatment dummy, the post-survey dummies, and an interaction between the treatment dummy and the post-period dummy, separately for catchment areas with only one or at least two private providers at baseline. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments and health and wellness center conversion. See Section 3.2 for details. Outcomes are obtained from our private provider surveys. The higher number of missing values in Columns (3)-(4) is because we only added these questions later in the baseline survey. See Data Appendix for details on variable definitions.

Table A20: IV Regressions

	Public Facilities		Private
	Quality	Person-Hours	Price
	(1)	(2)	(3)
CHO Assigned	0.694***	35.552***	
	(0.062)	(2.166)	
Lives in Village	0.017	10.776***	
	(0.051)	(3.082)	
Supplier Location: Chavand			-66.943***
			(11.332)
Supplier Location: Fateh Nagar			-11.393
			(16.797)
Supplier Location: Salumbar			48.367***
			(14.127)
Supplier Location: Udaipur			17.493*
			(9.740)
Supplier Lcoation: Bhabrana			125.179**
			(55.217)
Mean of Outcome	1.998	67.203	110.683
F-Stat	64.175	140.474	14.352
Observations	468	468	290

Notes: This table shows the first-stage results for the instruments that we use in the demand estimation. The regressions control for equipment index, medicine index, years of experience, and the number of years the provider has been working in the village. Robust standard errors are reported in parentheses. The sample in Columns (1)-(2) is restricted to public healthcare facilities and the sample in Column (3) is restricted to private providers.

Table A21: ANM Residence Location and Catchment Area Characteristics at Baseline

	ANM Lives in Village (1)
Distance to Public Health Clinic (in km)	-0.006
	(0.004)
Total Population	-0.000
	(0.000)
Elderly Population Share	-0.381
	(1.088)
Scheduled Caste Share	-0.085
	(0.859)
Scheduled Tribe Share	-0.061
	(0.211)
Female Share	3.351
	(4.330)
Literacy Rate	1.014
	(0.638)
Land Ownership Rate	0.031
	(0.283)
Employment Rate	0.260
	(0.503)
(Imputed) Consumption per Capita (in INR)	-0.000
	(0.000)
Outcome Mean	38.710
Observations	176

Notes: This table shows the effects of catchment area characteristics on subcenter person-hours. Robust standard errors are reported in parentheses.

Table A22: Demand Estimates Using Alternative Price IVs

Parameters	Estimate	SE
Quality Index (in Standard Deviations)	0.404	(0.705)
Quality × Non-Poor	0.140	(0.756)
Person-Hours (in 10 Person-Hours)	0.279	(0.101)
Price (in INR 100)	-1.056	(0.535)
Price × Non-Poor	0.720	(1.377)
Distance (in km)	-0.469	(0.174)
Distance × Non-Poor	0.079	(0.300)
Equipment Index	0.092	(0.265)
Medicine Index	0.454	(0.378)
Years of Experience	0.001	(0.009)
Years Working in the Village	0.003	(0.011)
Private	-0.501	(0.466)
PHC	-0.343	(0.482)
σ	0.957	(2.409)
Constant	-3.941	(0.827)

Notes: This table reports the results from the estimation of the demand model using alternative instruments to address price endogeneity. Instead of using supplier location fixed effects, we use Hausman-style instruments based on average private provider prices in neighboring markets.

Table A23: Model Fit

Micro-Moments	Data	Model
$\mathbb{E}[poor_i \vert \{i \text{ chooses a subcenter}\}]$	0.415	0.420
$\mathbb{E}[poor_i \vert \{i \text{ chooses a PHC}\}]$	0.343	0.350
$\mathbb{E}[poor_i \vert \{i \text{ chooses a private provider}\}]$	0.286	0.284
$\mathbb{E}[lives\ in\ PHC\ location_i \vert \{i \text{ chooses a PHC}\}]$	0.570	0.570
$\mathbb{C}(p_j, p_{k(-j)} \vert j, k \neq 0)$	0.040	0.034

Notes: This table reports the observed and predicted micro moments in the data and demand model. The first four micro moments come from the CHIP household census data. The last micro moment comes from our household survey.

Table A24: Differences in Treatment and Control Group Characteristics under Optimal Assignment Rule

	Optimal Assignment (1)
Log Distance to PHC	0.101*
	(0.057)
Total Population (in 1,000)	0.088***
	(0.021)
1 Private Provider at Baseline	0.126*
	(0.072)
Poverty Share	0.026
	(0.194)
Subcenter Quality Index at Baseline	-0.463***
	(0.083)
Outcome Mean	0.513
Observations	187

Notes: This table shows which catchment area characteristics predict that the treatment group subcenter would receive a CHO under the optimal assignment rule.

Table A25: Counterfactual Analysis by Poverty Status

Counterfactuals	Market Shares: Subcenter (1)	PHC (2)	Private (3)	Average Quality (4)	Δ All-Age Mortality Rate (in %) (5)
Panel A: Poor Households					
1) Baseline	0.104	0.283	0.071	1.049	
2) Full Treatment Effect	0.268	0.240	0.064	1.365	-0.112
3) Only Increase in Subcenter Quality	0.130	0.276	0.068	1.152	-0.036
4) Only Increase in Subcenter Person-Hours	0.224	0.253	0.060	1.139	-0.032
5) No Effects on Private Providers	0.270	0.241	0.057	1.346	-0.105
6) Private Sector Ban	0.112	0.295	0.000	1.008	0.015
7) Private Sector Ban (+ CHOs)	0.286	0.249	0.000	1.331	-0.100
8) Actual	0.201	0.257	0.066	1.232	-0.065
9) Random	0.193	0.259	0.067	1.221	-0.061
10) Random (Within-Markets)	0.194	0.259	0.067	1.221	-0.061
11) Rule: Poorest Catchment Areas	0.197	0.263	0.066	1.271	-0.079
12) Rule: Areas with 1 Private Provider	0.194	0.260	0.069	1.232	-0.065
13) Rule: Largest Catchment Area Population	0.203	0.260	0.069	1.232	-0.065
14) Rule: Furthest from PHC	0.185	0.270	0.066	1.237	-0.066
15) Rule: Subcenter Quality at Baseline					
16) Optimal	0.212	0.260	0.067	1.290	-0.085
17) Optimal (Within-Markets)	0.211	0.256	0.067	1.261	-0.075
Panel B: Non-Poor Households					
1) Baseline	0.096	0.363	0.119	1.399	
2) Full Treatment Effect	0.261	0.304	0.111	1.671	-0.096
3) Only Increase in Subcenter Quality	0.128	0.352	0.115	1.492	-0.033
4) Only Increase in Subcenter Person-Hours	0.210	0.326	0.104	1.450	-0.018
5) No Effects on Private Providers	0.265	0.307	0.097	1.637	-0.085
6) Private Sector Ban	0.110	0.395	0.000	1.360	0.014
7) Private Sector Ban (+ CHOs)	0.294	0.328	0.000	1.635	-0.084
8) Actual	0.193	0.328	0.114	1.560	-0.057
9) Random	0.187	0.330	0.115	1.549	-0.053
10) Random (Within-Markets)	0.189	0.330	0.115	1.549	-0.053
11) Rule: Poorest Catchment Areas	0.178	0.336	0.114	1.518	-0.042
12) Rule: Areas with 1 Private Provider	0.189	0.329	0.119	1.572	-0.061
13) Rule: Largest Catchment Area Population	0.197	0.329	0.119	1.572	-0.061
14) Rule: Furthest from PHC	0.177	0.342	0.114	1.557	-0.056
15) Rule: Subcenter Quality at Baseline					
16) Optimal	0.206	0.330	0.115	1.615	-0.077
17) Optimal (Within-Markets)	0.202	0.326	0.115	1.591	-0.068

Notes: This table presents the results of the counterfactual analysis, separately for poor and non-poor households. The different scenarios are as follows. Row (1): the baseline model. Row (2): full treatment effect in which subcenter quality and person-hours increase and private providers improve their quality in all subcenter locations. Row (3): only increase in subcenter quality, no change in subcenter person-hours or private provider quality. Row (4): only increase in subcenter person-hours, no change in subcenter or private provider quality. Row (5): increase in subcenter quality and person-hours, but no change in private provider quality. Row (6): ban on private providers, no change in subcenter quality and person-hours. Row (7): ban on private providers and increase in subcenter quality and person-hours in all subcenter locations. Row (8): 96 out of the 187 sample SHCs receive a CHO as per the observed government assignment. Row (9)-(10): average outcomes across 100 random allocations of the 96 CHOs within the same markets or across markets. Row (11)-(15): rule-based assignments based on the stated priority criteria. Row (12) first chooses areas with one private provider and then randomly chooses another clinics until 96 CHOs are assigned. Row (16)-(17): Optimal allocation based on the objective to maximize the decline in all-age mortality rates. Row (15) only reallocates CHOs within markets. Columns (1)-(3) show the average market shares for subcenters, PHCs, and private providers. The market share of the outside option is omitted. Column (4) reports the average healthcare quality of the chosen provider, with quality defined as 0 if the outside option is chosen. Column (5) reports the predicted relative decline in all-age mortality rates based on the changes in average quality.

Table A26: Heterogeneity by CHO Gender

	Number of Patient Visits	Number of Hypertension Patient Visits	Number of Acute Heart Disease Patient Visits	Maternal & Child Health Services Index	Any Death	All-Age Mortality Rate (IHS)	Any Elderly Death	Elderly Mortality Rate (IHS)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post × Female CHOs	206.772***	6.342***	0.013	0.002	-0.026*	-0.023	-0.024*	-0.063
	(10.500)	(0.765)	(0.009)	(0.015)	(0.015)	(0.017)	(0.013)	(0.040)
Treatment × Post × Male CHOs	221.092***	6.165***	0.030***	-0.005	-0.031**	-0.038**	-0.037***	-0.114***
	(9.448)	(0.642)	(0.008)	(0.014)	(0.014)	(0.015)	(0.012)	(0.036)
p-value: Coef 1 = Coef 2	0.131	0.811	0.055	0.634	0.684	0.173	0.156	0.063
Female CHOs:								
Treatment Group Mean (Pre-Periods)	239.142	3.288	0.043	0.032	0.379	0.314	0.242	0.685
Counterfactual Treatment Group Mean (Post-Periods)	370.612	8.464	0.041	0.145	0.369	0.307	0.253	0.717
Observations	1,644	1,644	1,644	1,644	1,644	1,644	1,644	1,644
Male CHOs:								
Treatment Group Mean (Pre-Periods)	240.348	3.543	0.036	0.039	0.370	0.316	0.249	0.704
Counterfactual Treatment Group Mean (Post-Periods)	371.818	8.719	0.035	0.152	0.360	0.309	0.260	0.737
Observations	3,250	3,250	3,250	3,250	3,250	3,250	3,250	3,250
Control Group:								
Control Group Mean (Pre-Periods)	238.718	3.595	0.037	0.012	0.368	0.314	0.238	0.682
Observations	4,892	4,892	4,892	4,892	4,892	4,892	4,892	4,892

Notes: This table shows the effects of CHOs on healthcare services and health outcomes by CHO gender. The regression coefficients are estimated by regressing the outcome on interactions between the CHO assignment dummy and the post-periods dummy and CHO gender, and year and subcenter fixed effects. Standard errors are clustered at the subcenter level. Subcenter-level weights for the control group are constructed by inverse probability weighting using propensity scores based on inputs used for CHO assignments. See Section 3.2 for details. In Columns (1)-(8), the sample consists of 4,909 subcenters and the sample period covers Q2 2020 until Q1 2024. In these columns, the outcomes are at the subcenter level and are obtained from the Pregnancy, Child Tracking and Health Services Management System. See Data Appendix for details on variable definitions.

B. Data Appendix

Outcome Variables - Administrative Data

- *Number of Patient Visits:* the total number of patient visits received by the ANM and the CHO.
- *Number of Acute Heart Disease Patient Visits:* the total number of visits from patients diagnosed with acute heart disease.
- *Number of Stroke Patient Visits:* the total number of visits from patients diagnosed with stroke.
- *Number of Epilepsy Patient Visits:* the total number of visits from patients diagnosed with epilepsy.
- *Number of COPD Patient Visits:* the total number of visits from patients diagnosed with chronic obstructive pulmonary disease.
- *Number of Hypertension Patient Visits:* the total number of visits from patients diagnosed with hypertension received by the ANM and CHO. This includes newly and previously diagnosed patients.
- *Number of Diabetes Patient Visits:* the total number of visits from patients diagnosed with diabetes received by the ANM and CHO. This includes newly and previously diagnosed patients.
- *Maternal & Child Health Services Index:* standardized index that consists of: the number of pregnant women with at least four prenatal care visits, the number of pregnant women who were given 360 calcium tables, the number of pregnant women who received their first tetanus shot, the number of children aged 9-11 months who have been fully immunized, and the number of women getting a post partum checkup in the first 7 days.
- *Any Death:* An indicator variable that is equal to one if at least one death occurred in the corresponding quarter in the catchment area of the subcenter. Similar definitions are used for any elderly deaths and any other-age-group deaths.
- *Number of Deaths:* the total number of deaths that occurred in the corresponding quarter in the catchment area of the subcenter. Similar definitions are used for number of elderly and other-age-group deaths.
- *Mortality Rate:* the total number of deaths that occurred in the corresponding quarter in the catchment area of the subcenter per 1,000 individuals in the catchment area. For the elderly mortality rate, we divide the number of elderly deaths by the number of elderly individuals in the catchment area. The number of elderly individuals is calculated by multiplying the catchment population by 8.8%, the average elderly population share in our sample villages in the SHRUG data. Data on the catchment population

are obtained from the Health and Wellness Center Portal. A similar definition is used to define the mortality rates for other age groups.

- *Mortality Rate (IHS):* the inverse hyperbolic sine of the mortality rate.
- *Chronic Elderly Deaths:* aggregates the following cause-of-death categories: heart disease, HIV/AIDS, cancer, tuberculosis, neurological disease, and 'other-chronic' deaths.
- *Acute Elderly Deaths:* aggregates the following cause-of-death categories: diarrhea, respiratory infections, fever, dengue, encephalitis, malaria-plasmodium vivax, malaria-plasmodium falciparum, kala azar, and 'other-acute' deaths.
- *Accident-Related Elderly Deaths:* aggregates the following cause-of-death categories: trauma/accident/burn cases, suicide, and animal bites.
- *Unknown-Cause Elderly Deaths:* the number of elderly deaths from unknown causes.

Outcome Variables - Household Survey Data

- *Any Symptoms:* an indicator variable that is equal to one if the household member had at least one health symptom in the past 30 days.
- *Medical Expenses:* the total amount the household member spent on healthcare visits, medicines, tests, and transport to healthcare providers in the past 30 days.
- *Any Hospitalization:* an indicator variable that is equal to one if the household member has been hospitalized (spent at least one night in a hospital or clinic) in the past 6 months.
- *Hospital Days:* the total number of days the household member has been hospitalized in the past 6 months.

Patient Exit Surveys

- *Overall Satisfaction:* obtained from the following survey question: "On a scale of 1 to 5, how satisfied are you/is the patient with the provider? (1 is least satisfied and 5 is most satisfied)".
- *Number of Questions Asked:* the number of questions the provider asked to the patients based on patient self-reports.
- *Measured Blood Pressure:* an indicator variable that is equal to one if the blood pressure of the patient was measured during the healthcare visit.
- *Any Antibiotics:* an indicator variable that is equal to one if the patient was given antibiotics. Medicines were classified by the research team. Medicines names were collected by asking patients to show the enumerators all the medicines that were given to them by the healthcare provider.

- *Referred:* an indicator variable that is equal to one if the patient was referred to another healthcare facility.

Public and Private Provider Surveys

- *Number of Providers:* the total number of private healthcare providers in the catchment area.
- *Number of Patients:* the total number of patients seen by the private healthcare facility in the past 30 days.
- *Typical Fee:* obtained from the following survey question: "what are your normal fees for primary care, including medicine and consultation fees?"
- *Quality Index:* standardized index that consists of: length of medical degree, average checklist completion rate in the asthma vignette, and a dummy variable for whether the hypothetical patient in the asthma vignette received the correct treatment or was referred to another provider.
- *Length of Medical Degree:* the length of the highest medical degree of the provider in years. If the provider was enrolled in a degree program at the point of the survey, we assume that the provider will complete the degree.
- *Checklist Completion Rate:* the average checklist completion rate of the provider in the vignette.
- *Correct Treatment of Referral:* a dummy variable for whether the hypothetical patient in the vignette received the correct treatment or was referred to another provider.
- *Injection Rate:* the share of patients who received an injection from the provider in the past 30 days.
- *Antibiotic Dispensing Rate:* the share of patients who received antibiotics from the provider in the past 30 days.

C. Data Collection

We conducted in-person surveys with ANMs, CHOs, and private providers. For private provider surveys, we mapped all private providers in the catchment area of the subcenter at baseline and endline by surveying ANMs and two local shopkeepers. Once the mapping was complete, we attempted to survey each of the private providers. The survey collected information on the personal details of each provider, their medical knowledge through two vignettes (child dysentery and adult asthma), the number of patients in the last 30 days, the share of patients who received antibiotics and injections, average fees, and participation in training workshops.

Among subcenters that had at least one private provider in the catchment area at baseline, we further conducted a phone survey with 513 households. We obtained contact details through the list of registered pregnancies in the PCTS portal. To be included in our sample, the household needed to have at least one registered pregnancy in the last five years. Since 94% of pregnancies are registered in India according to data from the National Family Health Survey 2019–2021, this covers most households who had a pregnancy in the past five years. The household survey collected information on health outcomes and healthcare utilization for all household members.

We surveyed 95% of ANMs at baseline and 99% of ANMs at endline. ANMs who were not surveyed were on temporary leave. We managed to conduct a baseline survey with 71% of the private providers we mapped in the catchment area. The main reasons for noncompletion were temporary closures and refusals. Out of the private providers still operational at the point of the endline survey, we managed to resurvey 88%. For the household phone survey, we managed to reach 26% at baseline. Noncompletion was primarily due to incorrect phone numbers. Households that we surveyed at baseline and could not reach over the phone at endline we also attempted to survey in person. This helped us to increase the follow-up completion rate at endline from 54% to 90%. The phone and in-person completion rates do not differ by treatment.

D. Rollout of Health and Wellness Center Reform

The first part of the Health and Wellness Center reform was to convert subcenters and PHCs to Health and Wellness Centers. Subdistrict officials had to propose a fixed number of subcenters for conversion annually between 2018 and 2022. The minimum criterion for conversion was that the government must own the subcenter building. In some years, priority was further given to subcenters with electricity, running water, and good physical condition. The physical conversion of subcenters mostly consisted of the construction of another examination room and the painting of the walls in a bright yellow color for branding purposes. In our survey, 74% of ANMs said that a single additional room was built as part of the reform. Changes in electricity (2%), running water (3%), or equipment (2%) were rare. The reform was also supposed to increase the set of medicines available at the subcenters, but these changes had not yet been implemented when we conducted our endline surveys.

The reform officially increased the set of available services at subcenters from six to twelve services. The newly added services included basic oral, palliative, and mental healthcare. However, many of these services were not provided frequently during our study period due to insufficient awareness and limited availability of necessary equipment and medicines. We consider these additional services to be part of the strengthening of basic outpatient care.

The main element of the Health and Wellness Center reform was the posting of CHOs to subcenters. While ANMs have a two-year diploma, CHOs are required to have either a three-year degree in general nursing and midwifery or a four-year bachelor's degree in nursing. They further need to complete a six-month bridge course upon being hired. The primary role of CHOs is to provide basic adult outpatient care and screening for chronic diseases at the subcenter level. The screening for chronic diseases is done through outreach camps and by screening patients who visit the subcenters during a routine medical consultation. Once posted, a CHO is the designated team leader of the health worker team at the subcenter.

In our sample, the average age of newly hired CHOs is 28 years, and 64% are male. They are paid a fixed monthly salary of INR 25,000 (≈ USD 300) plus INR 15,000 (≈ USD 180) in performance-based incentives. They further need to complete a six-month bridge course upon being hired. The screening for chronic diseases is done through outreach camps and by screening patients who visit the subcenters during a routine medical consultation.

E. Screening Rates and Mortality Declines

We conduct back-of-the-envelope calculations to estimate how much the increase in screening rates contributes to the observed decline in all-age mortality. We use data from the Global Burden of Disease Study to examine how many deaths can be attributed to hypertension and diabetes. For 2021, the study estimates that 15% of deaths in India be attributed to hypertension and 9% can be attributed to diabetes. We then use our estimated treatment effects to predict how many of these deaths could have been averted due to the CHO postings. As shown in Table 3, we estimate that the CHOs increase the number of hypertension patient visits by 6.2 and the number of diabetes patient visits by 4.7. NFHS data further shows that 7.3% of the rural population in Rajasthan have undiagnosed hypertension and 4.5% have undiagnosed diabetes. Combining this with our treatment effects and an average population of 3,115 people, we calculate that the share of undiagnosed hypertension and diabetes patients decreased by 22% and 27%, respectively. Using data from CHO surveys, we further assume that the share of newly diagnosed hypertension and diabetes patients who regularly take their medicine is 91%. Finally, we use estimates on the effectiveness of hypertension and diabetes medicines from the medical literature to assess how much mortality rates would decline conditional on medicine adherence. For hypertension, Hickey et al. (2021) find that a patient-centered hypertension care model reduced all-cause mortality among hypertensive patients by 21% within three years in rural Kenya and Uganda. For diabetes, we use estimates from a meta-analysis that finds that metformin, the most commonly used diabetes medicine in our setting, leads to a reduction in all-cause deaths by 29% among diabetes patients (Monami et al., 2021). Taking all of this together, we estimate that all-age mortality could have declined by 1.27% due to the increase in screening for chronic diseases [0.15 * 0.22 * 0.91 * 0.21 + 0.09 * 0.27 * 0.91 * 0.29].

F. Cost-Effectiveness Analysis

We use the Marginal Value of Public Funds (MVPF) to calculate the cost-effectiveness of the CHO postings (Hendren and Sprung-Keyser, 2020). We start with the most conservative case in which we ignore the change in hospitalizations. For government costs, we account for government spending on CHO salaries and increased spending on medicines. Our analysis consists of 2,487 treatment group subcenters, covering a total of 7,752,343 people. Each of these subcenters received a CHO who gets a monthly salary of USD 480, including incentive-based payments.[81] We further assume that the government spends USD 0.24 on medicines per outpatient visit. Combining these estimates with our treatment effects on quarterly patient visits (Table 3, Column (1)), we find that total government costs in the two years

[81] We use the current exchange rate of 0.13 INR to 1 USD.

are equal to USD 29,676,874 [2,487 subcenters * USD 480 * 24 months + 2,487 subcenters * 215 patients * 8 quarters * USD 0.24].[82]

For private benefits, we follow Hendren and Sprung-Keyser (2020) and use USD 100,000 as the value of a statistical life year. Combing these estimates with our treatment effects (Column (3) in Table 4 and Column (3) in Table 5), we find that total private benefits in the first year are equal to USD 202,939,200 [2,487 subcenters * 8 quarters * 0.102 decrease in deaths per quarter * USD 100,000 value of statistical life year].

Taken together, these results in a Marginal Value of Public Funds of 6.84 [USD 202,939,200 / (USD 29,676,874)]. We can further account for the decline in hospitalizations by using results from Garg et al. (2022) for government and private out-of-pocket spending per hospitalization visit. Garg et al. (2022) estimate that average spending per hospitalization episode is equal to USD 276.28 across all facilities in the state of Chhattisgarh.[83] One-third of these costs are, on average, paid by the government and the remaining two-thirds are paid out-of-pocket by patients. Using these estimates and assuming that the hospitalization decline persists throughout our sample period, we calculate that the reform would pay for itself by reducing future government spending on hospitalizations (7,752,343 people * 0.017 percentage point decline in hospitalizations over the past 6 months * USD 91.17 government spending per hospitalization visit * 4 semesters ¿ USD 29,676,874).

Appendix Table A27 shows how the cost-effectiveness estimates vary with different assumptions regarding the decline in mortality and hospitalization rates. Column (1) shows our estimates if we ignore the change in hospitalizations. Column (2) assumes that hospitalizations did not permanently decline but just got delayed by one year. Column (3) shows estimates if we ignore the decline in mortality rates. Column (4) assumes that hospitalizations only declined by 6 months (the reference period of our household survey). Column (5) shows our preferred specification that assumed a decline in hospitalizations by one year. Finally, Column (6) shows that the reform would pay for itself if we assume that the decline in hospitalizations also remains in the second year.

Table A27: Sensitivity of Cost-Effectiveness Analysis

All-Age Mortality Decline:	2 Years	2 Years	No Change	2 Years	2 Years	2 Years
Hospitalizations Decline:	No change	1 Year (Delay)	1-Year	6 Months	1 Year	2 Years
Marginal Value of Public Funds	6.84	7.17	8.64	12.87	44.59	∞

[82]Minor differences in our calculations below are due to rounding errors in the converted dollar values.
[83]Chhattisgarh is a central state in India that is slightly poorer than Rajasthan.